HARVARD UNIVERSITY.

DEPARTMENT

OF

PHILOSOPHY.

STUDIES

FROM THE

Yale Psychological Laboratory

EDITED BY

EDWARD W. SCRIPTURE, PH.D.

Instructor in Experimental Psychology

I.

1892-1893

Issued on October 1st, 1893

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PREFATORY NOTE.

THE monographs contained in the following pages are the results of such investigations as reached a successful conclusion during the first year of the existence of the laboratory. The first of them is essentially a thesis accepted by the university for the degree of Ph.D.

In the computation of results it was not deemed advisable during the first year of our work to depart from the usual psychological method of taking the average deviation (or mean variation) as the mean of the absolute differences of the observed values from the arithmetical average of the observations. MV means the same as in most other psychological publications, i. e.

$$MV = \frac{v_1 + v_2 + \ldots + v_n}{n}$$

where $v_1, v_2, \ldots, v_n$ are the differences of the single observations from their average, taken without regard to sign, and n is the number of observations. We propose, however, in the future to follow with such modifications as necessary the methods of the science of measurement as developed by Gauss. The sign $^\sigma$ means thousandths of a second.

INVESTIGATIONS IN REACTION-TIME AND ATTENTION

BY

CHARLES B. BLISS, PH.D.

INTRODUCTION.

The work described in the following pages occupied the greater part of my time during the academical year 1892–93. As I was the first to carry on such experiments in the Yale psychological laboratory, a large part of my fall term was spent in preparing the apparatus and in developing a method which should serve for all future experiments. The result is a method for measuring reaction-time which is in some parts entirely new. In operation it is simple and accurate, having been built up step by step as the needs required. In the hope that the whole or parts of it will be of value to other laboratories, the description has been made as complete as seemed necessary.

In the experimental part of the work I am especially indebted to the following persons for valuable time which they have spent in the reaction-room. To Messrs. Thomas J. Lloyd, William I. Cranford and Joshua A. Gilbert of the Graduate Department, and to Mr. Joseph Roby, a member of the senior class in Yale College.

During the second term, Abraham Fisher, the laboratory steward, recorded all the experiments, thus leaving me free to do my own reacting. The advantage of doing my own introspective observing was an important one.

Dr. Scripture not only suggested the first problem but has always been ready to assist me in carrying out the experiments and in arranging the apparatus. In fact, parts of the apparatus were invented by him. One line of research was carried out at the suggestion of Professor Ladd, who has always shown a kindly interest in my work.

In the drawing of the diagrams valuable suggestions were received from Mr. Walter I. Lowe, a member of the Graduate Department.

Apparatus.

Apparatus for measuring reaction-time must furnish some means for giving the reactor a stimulus and for measuring the interval of time between the moment in which the stimulus is given, and that in which the reaction takes place. The time-measurement must be accurate to thousandths of a second and the person experimented upon must, so far as possible, be free from all influences which would distract his attention.

This last requirement was met by placing the reactor in a separate room, so constructed as to be free from light and sound. In the center of the building a room was finished off, twelve feet long, nine feet wide and nine feet high. Inside of this room a smaller one was constructed with a door and ventilator corresponding to those of the outer room. This inner room was supported on thick cushions of felt and rubber, the only connection with the outer room being heavy canvas around the doors and the ventilators for the purpose of holding back the sawdust with which the space between the two walls was filled. The door was likewise made double with beveled edges, like a safe door, so that it shut tightly against the canvas connecting the two rooms. A thick mat, made of hair felt and covered with cloth, was hung up over the door on the inside. This acted like a heavy curtain to check any sounds which might creep in around the door.

During the experiments the door of the dark room was not shut more than five or ten minutes at a time. For that period the ventilator could be kept closed without producing any bad effects. In the case of longer experiments it is proposed to open a ventilator in the floor and pass a current of air through the room by means of a blower. The ventilators can then be packed with wool or some other material, which will allow the passage of air, but effectually shut out all sound. The experiments described in this paper were all taken in the winter, and the temperature of the room was the same as that of the rest of the building. Very loud sounds in adjacent rooms can still be heard in the reaction-room. Heavy wagons, which occasionally pass along the street, jar the whole building and with it this room; the shaking can be felt but not heard. When the adjacent rooms are kept quiet, the reaction-room is free from sound. The reactor is thus practically removed from all external disturbances in sight or hearing.

There are two methods in use for measuring intervals of time to thousandths of a second, the graphic method and that of the chrono-

scope. The Hipp chronoscope is the most perfect piece of mechanism thus far constructed for recording such short intervals of time on a dial. An immense amount of time and labor has been spent in perfecting this chronoscope and in investigating its accuracy. In its most perfect form there is always a very large error in the results as they are read off from the dial. This error depends on the relative strength of the electric current passing through it and that of a spring which pulls back the armature when the circuit is broken. A control-apparatus must be used which consists of a hammer so arranged that it can be made to fall certain distances. The time required for this fall is carefully measured by the graphic method and the spring of the chronoscope adjusted until the chronoscope itself measures the time of fall with the same result. Other times are accurately obtained by correcting the recorded results. The chronoscope in one of its forms is then accurate only for times of about that length. G. E. Müller claimed[1] that Münsterberg's experiments contain a large error even though he had corrected his chronoscope by a control-hammer. That particular hammer was made to correct the chronoscope for intervals of 160 thousandths of a second, whereas many of Münsterberg's experiments gave times as high as half a second. Although Münsterberg seems to have avoided the error supposed, yet the danger is evident. A more elaborate control-hammer has been constructed by Wundt.[2] By means of this hammer correct times can be given up to 616 thousandths of a second. The mean variation of this hammer in 200 experiments was 1.04 thousandths of a second. The mean variation of chronoscope and hammer combined was also 1.04^{σ}. This, the best result which has yet been obtained from the chronoscope, is ten times as great as the mean variation of the graphic method in its simple form.[3]

In the graphic method a tuning-fork, kept in constant vibration by a current of electricity, is allowed to trace a curve on a revolving drum covered with smoked paper. This gives a representation of a period of time divided according to the rapidity with which the fork vibrates. Using a fork which vibrates one hundred times a second the drum is revolved with such rapidity that the single waves are so long that we make no error in estimating tenths of a vibration and so reading the results in thousandths of a second.

[1] Göttingische gelehrte Anzeigen, 1891, p. 398.

[2] Külpe and Kirschmann, *Ein neuer Apparat zum Controle zeitmessender Instrumente*, Phil. Stud. 1892 VII 145.

[3] Wundt, Physiol. Psych. 3 ed. II 282.

Now, given this tuning-fork curve, all that is needed is some method of registering alongside of it the exact instants at which the stimulus and the reaction occur. Fig. 1 shows the usual way in

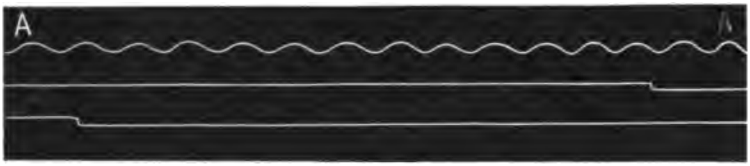


Fig. 1.

which this is done. The upper curve is drawn by the recording point of a tuning-fork which vibrates one hundred times a second. The other two lines are drawn by electro-magnetic time-markers. The current passing through one passes also through the key whose closing produces the stimulus. The current through the other passes through a key in the reaction-room. The movement of each key is thus recorded by a break in the straight line drawn by its time-marker. These points are then transferred to the tuning-fork curve by dropping perpendiculars from the points to the curve. The measure of the time which has elapsed between the movement of the two keys can then be counted off on the time-curve.

The objection to the use of this method in making a large number of experiments is that it takes a long time to drop the perpendiculars and that great variable errors are likely at the two points. These errors are increased by the fact that the time-markers must be adjusted so that they shall both touch the drum in the same perpendicular line. Moreover, the latent times of the markers may not be the same.

Fig. 2.

An improvement upon this method has been made by doing away with one of the markers. The same current is passed through both keys and through one time-marker. Fig. 2 shows a measurement made by this method. Closing the stimulus-key draws the lever of the marker toward its magnet, making a break in the straight line. Opening the key in the reaction-room breaks the circuit and allows the lever to fly back again, thus making a second break in the same straight line. These two points are transferred to the time-curve and the interval is counted off as before. The result is that the

number of lines on the smoked paper is reduced by one-third, allowing more experiments to be taken on the same paper and making the records easier to count. But more important than this is the fact that one large source of error is removed. The accuracy of the result no longer depends on the adjustment of the two markers so that they shall touch the drum in the same horizontal line.

The latent time for the two movements is generally different. And there still remains an error and a great loss of time in transferring the two points to the time-curve. What is wanted is some means of registering the interval directly upon the curve itself. This has been accomplished after trial of various methods. The first suggestion was to arrange an apparatus so that the stimulus-key when it was closed should start the curve and the reaction-key stop it by being opened. This was done by taking the fork from the drum and replacing it by one of the electro-magnetic markers. The current was run through the tuning-fork, the time-marker and the reaction-key, but it was short-circuited through the stimulus-key.

Fig. 3.

As long as the stimulus-key remained untouched, the marker did not vibrate; but as soon as it was touched, the record began. When the reaction-key was pressed, the entire circuit was broken and the record ceased. Such a record is shown in fig. 3.

At first sight this might appear to solve the problem but a closer examination shows that this is not the case.

Fig. 4 shows the way in which the Pfeil marker works. B is the battery, F the tuning-fork, M an electro-magnet, S a steel armature which serves as a spring; the lever is attached at O. This lever swings on a pivot at H; when the circuit is complete the vibration of the fork alternately makes and breaks the current at X. As soon as it is made the coil in the fork becomes a magnet, pulls the prongs inward and breaks the circuit. This demagnetizes the coil, the prongs fly back and the process is repeated indefinitely. But when the current is closed at X, the magnet M draws down the armature and its lever. When the current is broken in the fork the armature flies back carrying the lever with it. Thus the point P vibrates in unison with the fork.

Let CDE be a section of the curve which would be traced by the marker. From C to D the motion of the point comes from the spring that causes the armature to fly back. From D to E the motive force is a combination of the spring and the magnet. Now if the stimulus-key, which starts the current through the marker, is opened between C and D, no effect will be produced until the point D is reached for no current is passing through the circuit. Therefore the chances are one in two that the beginning of the movement will be too late by anywhere from 0 to 5^σ, the section of the curve from C to E being 10^σ with a tuning-fork which vibrates 100 times a second.

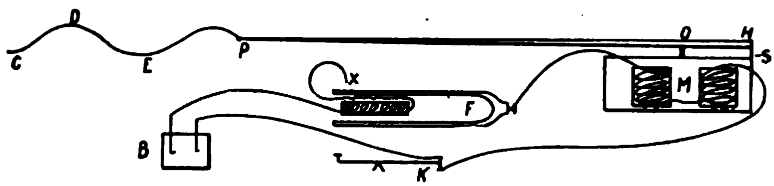


Fig. 4.

At the end of the interval the case is not quite so clear. If the reaction-key breaks the circuit between C and D, there will be no effect until the point D is reached. But, unlike the beginning, the effect will not be shown at D. For, when the marker is in motion the motive force, between D and E is a combination of the spring and the magnet. Near D the spring is stretched. The tension gradually decreases passing from a positive to a negative quantity somewhere below the middle point, while the force of the magnet gradually increases the nearer the armature approaches it. Therefore the effect of a break in the current is not shown until the magnetic component of the motive force reaches a certain strength in proportion to that of the spring before it is interrupted. Suppose that this takes place when three-fifths of the distance DE have been passed over, then the chances are seven to ten that the end of the interval will be registered anywhere from 0 to 7^σ too late.

In a large number of experiments these errors at the beginning and end would partially balance each other, but their presence would still be shown by the large mean variation. The beginnings would on the average be 1.25^σ too late and the ends too late by 2.45^σ. In a small number of experiments the results are not accurate beyond hundredths of a second. By using a fork which vibrates 500 times a second, the error would be reduced to 2^σ and by using a fork vibrating 2000 times a second, the method would be accurate to

thousandths of a second. This however is impossible since the time-markers are not delicate enough to record such rapid vibrations. Even if they were, the task of counting so many wave-lengths would render the method of no practical use.

The next step was to arrange the apparatus so that the time-marker vibrated continually in unison with the tuning-fork, but yet

Fig. 5.

so that closing the stimulus-key sent an additional current through the time-marker, which additional current was released by opening the key in the reaction-room. The result is shown in fig. 5. During the interval to be measured the vibration of the marker continues in a different line from that of the normal time-curve. Here we have the beginning and end of the interval accurately marked. By adjusting the strength of the two currents and the rapidity of the drum, this method will probably be quite successful. If so, it will be superior to any other method heretofore used. It is possible however only with the Pfeil marker which has a steel spring as shown in fig. 4 ; for it consists in a partial magnetization of the electro-magnet which draws the spring part way but still leaves room for it to be affected by the current passing through the tuning-fork.

This method was not used in the following experiments for the reason that a much better plan suggested itself. Instead of trying to change the curve to mark the beginning and end of the interval the apparatus was so arranged that closing the stimulus-key broke the primary current of a spark-coil and sent a spark from the tuning-fork to the metal drum through the smoked paper. Opening the key in the reaction-room likewise broke the same current and sent another spark through the smoked paper. Fig. 6 gives a specimen record taken by this method. Here we have both ends of the interval marked exactly, there is no time lost and no error arising from transferring the interval to the time-curve or in adjusting the markers on the drum.

Fig. 6.

The question now arises whether the spark occurs at the exact time the primary circuit of the spark-coil is broken. In a large dynamo the spark might be delayed several seconds, but in a small coil such as is used for this purpose there is no delay sufficient to affect the result. Fig. 7 is one of a series of experiments made to test this point. The time-curve represents hundredths of a second. The straight line was drawn by a pointer attached to the lever of a key, the least motion of which broke the primary current of the spark-coil, sending a spark from the key to the drum through the smoked paper. None of these experiments show any latent time.

Fig. 7.

Even if there were a latent time which could be detected by reading to ten-thousandths of a second, that would not affect the reliability of measurements taken by this means. For the same coil, under the same conditions, this latent time would remain constant. The spark which marks the end of the interval would be delayed just as much as the spark which marks the beginning; the interval between the two would be exactly the same as though both sparks were in their proper places.

Presupposing the accuracy of the tuning-fork, this method is absolutely accurate to thousandths of a second. In counting the records one falls into the habit of counting by threes, making use of the psychological fact that three things can be kept in consciousness at the same time as easily as one. In this way a paper containing twenty-two records of simple reaction-time can be counted in four minutes. This is a much shorter time than is required to record the readings of the Hipp chronoscope and correct them for the variable error.

The only justification for the use of the chronoscope lies in the supposition that it saves time. The method here described is much simpler and quicker, in addition to being absolutely accurate. For simple reaction-time this method is far more accurate and rapid than any hitherto described. But for longer times, such as association-time or discrimination-time, some easier method must be devised for counting the records. It is suggested that another time-marker be placed on the drum; this marker to be connected with a pendulum

or clock work which shall mark every third or fourth wave of the tuning-fork curve. With this assistance it will be possible to count long records quickly and accurately.

Two drums were used in these experiments, an electric drum and a König drum.

The electric drum consists of a brass cylinder mounted on iron brackets so as to turn on its axis. It is rotated by a small electric motor clamped to the right side of the table. Linen thread serves for a belt. The tuning-fork, or the marker is mounted on a carriage which moves along a track parallel to the drum by means of an endless screw turned by a crank at the left side of the table. One turn of the screw moves the marker on the drum one-quarter of an inch to the right or left. By regulating the strength of the current which runs the motor considerable variation can be produced in the speed of the drum, while a switch near at hand allows the motor to be stopped or started at will.

Two kinds of curves are produced by this drum. In fig. 8 the marker, or the fork itself, is kept vibrating on the drum all the time

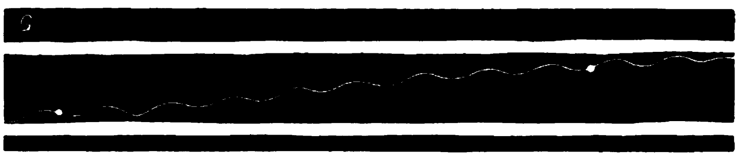


Fig. 8.

When a record is taken, the marker is quickly moved to the right or left by a turn of the screw. The record is made during this sidewise movement of the marker, the result being two white bands with the record on a curve passing obliquely from one to the other. The first spark comes from the stimulus-key, the second from that in the reaction-room.

At first it is somewhat difficult to make these two motions, the one with the right hand, the other with the left, just at the right instant, but a short practice enables one to do it with ease and accuracy. The whole difficulty, however, is removed by the multiple key to be described below. Using that, together with the electro-

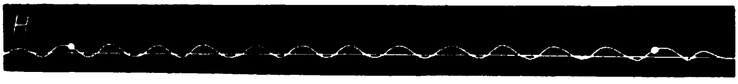


Fig. 9.

magnetic time-marker, we produce the curve shown in fig. 9. The marker remains stationary, simply drawing a white line around

the drum. Opening the stimulus-key starts the marker vibrating and an instant later gives the stimulus which is marked by a spark on the curve. As soon as the spark from the reaction-key has been recorded, the multiple-key is released, and the marker ceases to vibrate before the drum has made a complete revolution. The marker is then moved to the right or the left by a turn of the screw and another record is taken. A similar record on an ordinary drum is shown in fig. 10.

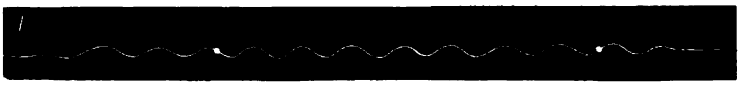


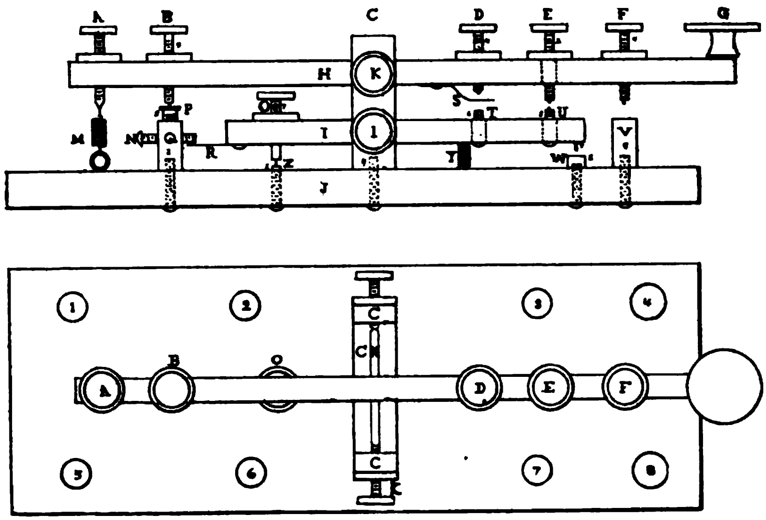
Fig. 10.

The other drum is constructed, as can be seen in fig. 13, so that every turn of the crank moves the drum itself half an inch to the right or left. In this case the standard holding the marker or the tuning-fork remains stationary. One hand turns the drum once around while the other closes the stimulus-key. The reaction always follows before the revolution is completed. For simple reaction-time this is much the better drum of the two. The records are always the same distance apart and can be made to begin in the same horizontal line on the drum, thus making the counting of the records much easier than those of the electrical drum which are scattered over the paper and are liable to be cut in two when that is removed from the drum. For other purposes the electrical drum is to be preferred.

It is evident from the various functions ascribed to the stimulus-key that something more than the ordinary telegraph-key is implied. In every case it is assumed that it produces the stimulus and records it at the same instant on the drum by means of a spark. In addition to this it sometimes starts the tuning-fork curve just before the stimulus and stops it just after the reaction has taken place. The necessity for some contrivance by which such things might be done was felt at the beginning of the work and led to the invention, by Dr. Scripture, of the multiple-key.

Figs. 11 and 12 show drawings of this key. It consists of two square bars of brass I and H rotating on small steel axles X held in place by check-screws K L passing through the upright parts C of a firm brass frame which is screwed to the wooden base J. One end of each bar is held by a spring M, Y ; the strength of M is regulated by the screw A. Besides this there are four other screws B, D, E, F, which pass through the upper lever, one of them F

being insulated from the lever by hard rubber. The screw B rests upon a small steel plate P, insulated by a hard rubber screw from the brass stanchion Q and connected by wire on the underside of the base with binding-post 5. None of the binding-posts are shown in the elevation because they would conceal important parts of the key. The screws B, D, F are connected through the brass rod, steel axles and upright support with a screw that passes through the base and thence by insulated wire with binding-post 4.



Figs. 11 and 12.

The screw B being adjusted so that the upper lever is level, the screw F regulates the amplitude of its movement. By means of this screw it makes contact with the brass stanchion V. The screw E which is insulated from this lever, is connected to post 2 by insulated wire running along the lever, down the standard through the base. The screw U with which it comes in contact is also insulated from its lever and connected in a similar way with binding-post 3. By means of screw D the copper spring S can be made to make contact with screw T which is insulated from its lever and connected with binding-post 6.

The lower lever is adjusted to a level position by screw O; it is insulated from its steel axle and together with screw O is connected with binding-post 7. The screw Z, with which it makes contact, is connected with binding-post 4. So are the brass stanchion Q and the screw N, passing through it, as well as the mercury cup W. The

screw E is so regulated that just before F makes contact with V the lever I breaks contact with the screw Z and immediately after makes contact again in the same circuit either through the screw N or the mercury cup W.

We have six contacts: three makes, two breaks, and one break followed by a make in the same current. One of these breaks, if used at all, must always come first and one of the makes, if used at all, must always come last. According to actual count this gives forty-four different ways in which currents can be passed through the key. When more than one current is being passed through the upper lever at the same time, care must be taken to have this lever connected with the same pole of all the batteries.

A few of the uses to which this key may be put will be mentioned here together with the contacts used in each case.

1. As an ordinary key where the contact is made by pressing down the key; circuit through E-U or F-V.

2. As an ordinary key in which the contact is broken by pressing down the key; circuit through B-P or O-Z.

3. To close two circuits at the same time; E-U, D-T.

4. To close to circuits at the same time and one an instant later; D-T, E-U, F-V.

5. To close one circuit and break another at the same time; E-U, O-Z.

6. To close two circuits and break a third at the same time; E-U, D-T, O-Z.

7. To break one circuit just before closing a second; B-P, E-U.

8. To break a circuit and an instant later close the same circuit again; O-Z, R-N, or I-W.

In the second method of recording reaction-time on the smoked drum, according to the arrangement of wires in fig. 13, the tuning-fork current is short-circuited at P-B while the key remains closed. When this contact is broken, the current passes around through the marker on the drum. A moment later the contact E-U closes a telephone-circuit which passes through the apparatus in the reaction-room and so produces the stimulus. But the primary current of the spark-coil is passing through O-Z. At the same instant in which the contact E-V is made, this contact O-Z is broken and a spark passes through the smoked paper. This primary circuit is made again at W in time to be broken a second time by pressure upon the key in the reaction-room. As soon as this reaction takes place the operator releases the multiple-key, the tuning-fork curve is short-

circuited again at B-P and the time marker ceases to vibrate. As the reaction always follows the contact E-U within three-tenths of a second, the key need not be kept open longer than is natural in slow movement.

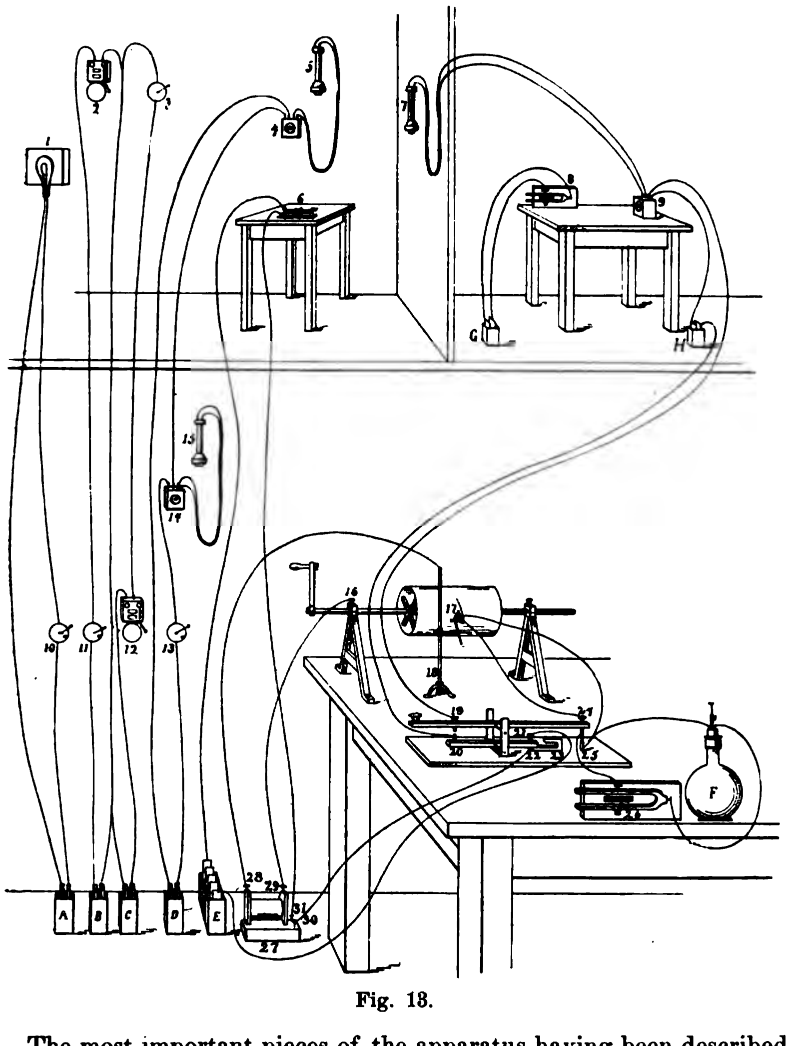


Fig. 13.

The most important pieces of the apparatus having been described in detail, its general arrangement can easily be understood from the diagram in fig. 13. The reaction-room is indicated above on the left. The room used for the production of sound stimuli is shown next to it, although it is situated in another part of the building so that

no sound from the loud tuning-forks can penetrate the walls of the reaction-room. The recording-room is on the floor below. These figures are all diagrammatic, being drawn to show the meaning of wires and apparatus rather than actual positions or proper proportions.

Taking the diagram from left to right, the first pair of wires belongs to an electric light circuit. The lamp 1, which was used in these experiments, was a miniature incandescent lamp of 6 c. p. By a switch, 10, the light could be turned on or off.

The next three wires connect two electric bells, 2 and 12, with the Leclanché elements B and C. Closing key 3 rings the bell in the recording-room. The gong being removed from bell 2 and the contact made permanent, closing key 11 only produces a click in the reaction-room. Otherwise the sound would be so loud as to distract the person reacting.

The next pair of wires forms a telephone-circuit by means of which the experimenter can talk freely with the person in the reaction-room. The switch 13 breaks this circuit during a series of experiments and so prevents any noise reaching the reaction-room through this telephone. This telephone connection is a new feature in reaction-time apparatus and its advantage cannot be overestimated. In some of the German laboratories the reactor and experimenter are in the same room, separated only by a cardboard partition. The reactor is thus influenced by every sound in the building, by the changing lights and shadows and by the noise of the chronoscope. In other laboratories the reactor is placed in a separate room in another part of the building. When the operator desires to speak to the reactor he must leave his work and go to this room, often breaking up the whole series and producing more or less distraction. With this arrangement the reactor is in a dark room, free from sound. By a turn of the switch he can hear even a faint whisper from the experimenter.

Next on the diagram comes a one-inch Ritchie spark-coil ; 31 and 30 are the poles of the primary circuit. The current from battery E, consisting of two to four Grove cells, passes through the closed key 6 in the reaction-room and through the contact 21-22 in the multiple-key (O-Z of fig. 11). When the key is pressed down the circuit is broken at 22 and closed immediately after at 23. In practice it was found better to use the mercury cup W, fig. 11, for this second contact as the contact with the copper spring and iron screw R, is a sliding contact liable to produce additional sparks and thus to con-

fuse the record. The mercury must be kept covered with water as alcohol takes fire with a current of the size used.

The poles of the secondary coil are 28 and 29. One was connected with the brass cylinder of the drum by being attached to the iron frame at 16, the other with the point which marks on the drum. When the electro-magnetic time-marker was used, a light aluminium point was substituted for the ordinary straw or quill point. Every time either key is pressed a spark passes from the marker to the drum through the smoked paper scattering the smoke and making the white dots shown in figs. 6–10.

Numbers 19 and 20 are two ends of a second telephone circuit. H is the battery, 9 the transmitter and 7 the receiving telephone. Before the transmitter there stands a tuning-fork 8 run by the battery G. When the multiple-key is pressed down, this telephone circuit is closed at 19-20 and the tuning-fork is heard in the telephone in the reaction-room. At the same instant a spark is made on the drum by the breaking of contact 21-22. The strength of this sound was regulated by passing the telephone-current through a resistance-board, not shown in the diagram. For purposes of simple reaction it was not necessary to use tuning-fork 8. The short, sharp click made in the telephone by closing the circuit at 19-20 was sufficient. By changing the resistance in the telephone-circuit this sound could be varied from one too weak to be heard to one too loud to be endured. It was found necessary to run the wires of this circuit from the recording-room to the reaction-room without allowing them to come near any other wires which were in use at the same time. Otherwise sounds were produced in the telephone by induction from the currents in those wires.

During the latter half of the experiments the two receiving telephones, 5 and 7, in the reaction-room were each connected with both transmitters, 4 and 9. By this arrangement the sounds from the recording-room and from the sound-room were heard in each telephone. One of them was fixed by rods and clamps in such a position, that the right ear of the person experimented upon naturally rested against it. The other was held to the left ear by the left hand, while the right hand was free to manipulate the reaction-key.

A still better plan would be to use a head telephone with a receiver at each ear. This would always be in place, leave both hands free and allow the person reacting to take the most comfortable position and to move about instead of keeping the body in one fixed position. However, as a series of experiments never lasted over five

minutes, the disturbance from the act of holding one of the telephones cannot have been very great.

The remaining wires shown in the diagram all have to do with the time-curve. The tuning-fork 26 is run by a dip-battery F. This current passes through the contact 24-25 when the key is closed as shown in this figure. When the key is opened, this contact is broken and the current passes around through the electro-magnetic time-marker 17 communicating to its lever the vibration of the tuning-fork.

In most of the experiments made with the hand-drum this time-marker was not used. The fork itself was placed on the standard and allowed to vibrate continuously on the smoked paper.

In addition to the wires shown in the diagram another pair was used to connect an electro-magnet in the reaction-room with a battery and switch in the recording-room.

All of these wires are part of a system of wires running through the whole building. At first seven wires were laid from each room to a switch-board in a central position. This number not being sufficient for the currents required between the reaction and recording-rooms seven more were laid to each of these rooms. All of these wires where the resistance is of small importance, such as the telephone and bell circuits, are number 16 B. and S. office wire. For the electric light and the primary circuit of the spark-coil, where stronger currents are required, number 10 heavily insulated wire was put in. These were run directly from the reaction-room to the recording-room independent of the switch-board.

Experiments in reaction-time.

In all the experiments the stimulus to which the person in the dark room reacted was a sound produced in the telephone.

A warning click was given on the bell in the reaction-room just before each experiment. Experiment has shown that when the interval between warning and stimulus is always the same the mind is soon able to estimate the interval correctly and always reacts just at that time whether it hears the stimulus or not. Therefore this warning cannot be produced by any mechanism connected with the drum but must be given by the voluntary act of the experimenter. The effect of this warning on the reaction-time depends on the interval between the warning and the stimulus. If the interval is too short there is not time enough to concentrate the attention and the warning hinders the reaction instead of helping it. If the time is too

long the effect dies away, as the mind is not able to keep its maximum tension for more than one or two seconds. L. LANGE[1] mentions about 2 seconds as the best interval. WUNDT[2] places it at 2.5 ; Estel[3] says 2.25; MEHNER[4] and GLASS[5] agree on 2.5. BERTELS[6] found that it took the mind 2⅜ seconds to reach the maximum degree of attention. The interval used in these experiments was 2½ seconds, as nearly as the experimenter could estimate it by counting.

Much also depends upon the interval between the successive experiments. If it is too long the series covers too much time. Changes in the mental and bodily condition of the experimenter come in to change the reaction-time. If the interval is too short there is not time to recover from the preceding experiment. About fifteen seconds was the interval between the successive experiments in the present case. As reaction requires close attention, not more than twenty-five experiments can be taken at one sitting without showing marked effects of fatigue. In the larger part of these experiments the number was limited to twenty-two and at least five minutes intervened between successive series. Seldom were more than five series taken at one time.

An important point in which there is less agreement is that of the rejection from the records of unusually long or short times. These have usually been regarded as errors and ascribed to two sources, to a faulty action of the electro-magnet in the Hipp chronoscope, and to inattention on the part of the reactor. The present apparatus eliminates the former error but the latter still remains. Inattention may give long times and the person may react before he actually hears the stimulus. By keeping a careful watch most of these cases will be noticed on the spot and be rejected without question as errors. But still the tables will contain an occasional long or short record which largely affects the average of the series. What shall be done with these cases? Some have refused to omit any, claiming that individual differences disappear in the final average. Most writers use their judgment in each particular case and reject all records which they think unduly affect the results. It is always hard to draw the line between normal attention and the next grade below it, to decide

[1] *Beiträge zur Theorie der sinnlichen Aufmerksamkeit*, Phil. Stud. 1888 III 492.
[2] Physiol. Psych. 3 ed. II 361.
[3] *Neue Versuche über den Zeitsinn*, Phil. Stud. 1885 II 37.
[4] *Zur Lehre vom Zeitsinn*, Phil. Stud. 1886 II 560.
[5] *Kritisches und Experimentelles über den Zeitsinn*, Phil. Stud. 1888 IV 454.
[6] *Versuche über die Ablenkung der Aufmerksamkeit*, Inaug. Diss. Dorpat 1889.

which are correct reaction-times and which are errors. The purpose of the experiments may have an influence in deciding this question. For instance, if it is to get the average reaction-time of a certain person in a series lasting five minutes, then more marked cases of inattention would be expected and let pass unchallenged than if the purpose was to detect the influence of a slight distraction on the reaction-time. In the former case the variation in attention is the quantity to be measured. In the latter case it is desired to eliminate all variations in the attention save that due to the one cause whose effect is being investigated.

In the present instance every record was rejected which seemed to the *reactor* to be a mistake. His opinion was always written down before he left the reaction-room and before he knew what the figures were. After that the criterion for rejecting readings was that laid down by HOLMAN.[1] "Take the mean and the average deviation of the observations, omitting the doubtful one. Find the deviation of that one from the mean. Then reject the observation if its deviation is greater than four times the average deviation." This is an arbitrary criterion and does not imply that all records rejected by it are errors. It means rather that in the small number of records they would have undue influence and that the average without them will be nearer the truth than if they were included.

In every case excepting the few series where the names are given in the table the writer was the person experimented upon.

EXPERIMENTS SHOWING THE INFLUENCE OF SENSATIONS OF LIGHT UPON THE TIME OF SIMPLE REACTIONS TO SOUND.

The first problem undertaken was the investigation of the influence of the presence in consciousness of different colored lights upon the time of reaction to sound-stimuli. It was suggested by the results of FÉRÉ's experiments with the dynamometer.[2] He found that with hysterical patients different colored lights had different dynamogenic effects, red being most effective and violet the least. If the energy with which the muscles can be contracted varies with the appearance in consciousness of different colored lights it seems probable that there should be a similar effect upon the rapidity with which they can be moved.

[1] HOLMAN, Discussion of the Precision of Measurements, New York 1892, p. 30.
[2] FÉRÉ, Sensation et mouvement, Paris 1887.

These experiments are not to be confused with those conducted by TITCHENER in Wundt's laboratory where different colored lights served as the stimulus.[1] The effect of a steady influence might be detected when the effect of a momentary influence was too small to be measured. We certainly have a different tone of feeling when looking at a red light from that which we have when looking at a green light.

The different colored lights were produced by colored gelatine films between two pieces of glass placed before the box containing the electric light.

Three hundred experiments were made upon this point, but they must be regarded as preliminary and negative. They were made upon six different persons all of whom were without practice in reacting, and, as they were taken before the apparatus was completed, the method illustrated in fig. 3 was used, the error of which has already been pointed out.

The results show no difference for the different colors within the limits of error and none between those taken in the dark and those in the light. However, in all cases they show the effect of practice on reaction-time. Table I brings this out in the case of two persons, the first one of whom reacted several thousand times in the interval

TABLE I.

Name	Date	R	MV	n
C.B.B.	Nov. 13.	183	34	90
"	Mar. 31.	140	13	236
J.A.G.	Dec. 1.	250	36	59
"	Jan. 30.	161	22	28

R, reaction-time.
MV, mean deviation.
n, number of experiments.

between the two dates given while the second reacted only a few times between the dates for which his reactions are compared. In the

[1] *Zur Chronometrie des Erkennungsactes*, Phil. Stud. 1893 VII 140.

case of the first person an average of 90 experiments taken on Nov. 13, gave a reaction-time of 183^σ and an average deviation from that average of 34^σ. On March 31 an average of 236 experiments gives a reaction-time of 140^σ with an average deviation of $13.^\sigma$ Thus showing not only a great falling off in the time but also a great increase in the regularity of the experiments.

A like result is shown in the record of the second person.

From these figures it is plain that we are not to expect differences due to small changes in the conditions of the experiments to show themselves until the person experimented upon has had some practice.

One other set of experiments was taken before the apparatus was accurate to thousandths of a second. These were all made upon one man, the object being to see whether, with the degree of accuracy then obtained, any difference could be detected between the time of reactions taken in the dark and those taken in the light. The averages of the separate series are given in table II. The final average of 115 experiments in the dark gives a result of 170^σ with a mean variation of 23^σ. The 74 experiments taken in a white light

TABLE II.

Disturbance	R	MV	n
None	170	23	115
White light	177	30	74
Red light	175	25	20
Green light	160	15	20

give a time of 177^σ with a mean variation of 30^σ. The 20 experiments in red light give a time of 175^σ with a mean variation of 25^σ and the 20 experiments in green light give a time of 160^σ with a mean variation of 15^σ.

After the apparatus was correct to thousandths of a second, another attempt was made to detect a difference between the time of reactions in the dark and those in the light. A series of twenty or thirty experiments was taken in which the light was turned on in the mid-

dle of the series. Table III gives the average of five experiments before the light was turned on and five immediately after. The experiments were taken upon seven different persons only two of whom had had experience in reacting and only one of whom had had practice that year. Considering this and also the fact that only five series were taken upon any one person, too much confidence must not be placed in the results. It would be very easy to say that they show the distracting influence of the light; for in the case of each individual, save one who had had no practice and upon whom only one series was taken, the average of the reactions in the light is longer than those in the dark.

TABLE III.

Person	D	MV	*n*	L	MV	*n*	L - D
D. W. L.	148	15	15	150	11	15	+2
C. B. B.	147	7	25	151	9	25	+4
E. W. S.	184	28	25	191	24	25	+7
J. M. M.	159	12	5	191	24	5	+32
W. I. C.	146	16	20	156	9	20	+10
K. M. W.	119	30	10	139	16	10	+20
J. A. G.	181	28	5	171	15	5	−10
Weighted mean	153	18	105	160	14	105	+7

D, reaction-time in darkness.
L, reaction-time in light.

The final average of the 210 experiments is 153^{σ} for the dark with a mean variation of 18^{σ} and for the light 160^{σ} with a mean variation of 14^{σ}. But later and more reliable experiments show that the mere presence in the field of vision of this steady light would not produce that effect. A glance at the original records throws some light on this point. In eight of the series the first reaction after the light was turned on was unusually long. In the nature of the case these

records could not be rejected, for though there is a possibility that they may be due to inattention yet it is far more probable that they are the very thing we are looking for. When the light is turned on it startles the person for an instant and so increases the reaction-time. Table IV shows this fact very clearly. It contains the first, second, third, fourth and fifth experiments before and after the light was

TABLE IV.

Person	Dark					Light					n
Number of exper.	*5.*	*4.*	*3.*	*2.*	*1.*	*1.*	*2.*	*3.*	*4.*	*5.*	
D. W. L.	181	153	141	128	140	149	171	144	146	139	3
C. B. B.	139	150	132	153	150	158	145	142	156	153	5
E. W. S.	175	179	172	147	202	178	187	192	177	223	5
J. M. M.	165	151	155	188	139	248	194	167	166	179	1
W. I. C.	185	154	139	158	140	198	150	142	163	123	4
K. M. W.	114	131	103	135	108	151	122	118	136	167	2
J. A. G.	183	148	123	299	153		197	177	176	134	1
Weighted mean	154	156	142	153	155	165	162	155	160	166	

turned on in each series, so arranged as to show the averages for the first, second, third, fourth and fifth experiments in the case of each individual and also the final average of all together. In the final average and in the case of most of the individuals, notably in the case of those who were practiced, the first experiment after the light was turned on was longer than the other and enough so to affect the averages in table III.

When the light was turned on, no care was taken to have it come exactly half way between two experiments. Sometimes it would be nearer the one before, sometimes nearer the one which followed. This may explain the reason why some of the series show no distraction. The light was turned on in the early part of the interval

and the person had time to accommodate himself to it before he heard the stimulus. Doubtless if the interval between the moment of turning on the light and that of the reaction immediately following it were carefully measured it would be found that this first reaction would be lengthened the more the smaller this interval.

The next attempt to detect a difference between the time of reactions in the dark and those in the light was made later on in the year, after considerable practice. In the experiments already described the electric lamp was carefully concealed in a box, the sides of which were lined with tin reflectors, so that the person sitting at the reaction-table saw only a brightly illuminated square of white card-board in the back of the box. Since the effect was so slight the box was dispensed with in these later experiments and the lamp hung suspended in full view. The method was the same as in the former case, except that the order of light and dark was reversed in some of the series to eliminate any effects from fatigue or acceleration which might enter into a series of twenty experiments.

The total number of experiments was 207, consisting of 97 in the dark and 108 in the light. Of the ten series, six gave a slightly longer time for those in the light while the other four gave an equally small difference in favor of those in the dark. The final averages are: for those in the light, 138^{σ} with a mean variation of 12^{σ} and for those in the dark 136^{σ} with a mean variation of 10^{σ}, thus showing a difference of only 2^{σ} both in the reaction-time and in the mean variation, a difference which is practically zero. These figures then warrant the statement that *the difference between reaction-time in the dark and reaction-time with the eyes fixed upon a bright steady light is very small compared with the constant variation due to subjective changes in the condition of the person experimented upon.*

After it was found that the presence of a steady intensely bright light in the field of vision produced no variation in the reaction-time which could be detected, it was decided, at the suggestion of Professor Ladd, to study the effect of a moving light.

The small incandescent lamp was suspended from the ceiling by a flexible cord about six feet long. Just above the lamp a piece of soft iron was fastened to the cord. This iron served two purposes: it made the lamp swing steadily and allowed it to be held in position by an electro-magnet fastened to one end of the room. The electric light current and that of the electro-magnet were then both passed through the same switch in such a way that one movement of the lever broke the electro-magnet current and turned on the light.

Ten experiments were taken in the dark ; then the operator turned the switch and the person experimented upon had a steadily swinging light in his field of vision. In addition to the moving light there was always a great variety of moving shadows and changing intensities as the lamp swung past the wires, standards and other parts of the apparatus. When the light was turned on, the eye invariably followed the lamp for one or two minutes, after which the shadows came into consciousness, then the objects which produced the shadows and finally other things in the room.

The amplitude of the motion was regulated by moving the electromagnet. During part of the experiments the lamp commenced to swing through an arc of two meters, which gradually diminished during the experiment to half a meter. In the rest of the experiments the arc was half a meter at first and the lamp gradually approached a state of rest. Had the results warranted, a more careful determination of this amplitude would have been made. In these experiments the order of light and dark was changed to eliminate other influences. It was of course impossible to stop the lamp swinging in the middle of a series, but the light could be turned out.

The mean variations are fairly low and regular. Therefore the results may be regarded as quite trustworthy. In twelve out of the sixteen series the reaction-time while looking at the swinging light was from 2^σ to 20^σ longer than the reaction-time in the dark. The final averages for those in which the long swing was used were : in the dark, 137^σ with a mean variation of 13^σ ; in the light, 142^σ with a mean variation of 11^σ. For those with the short swing they were : dark, 142^σ, mean variation 12^σ; light, 147^σ, mean variation 12^σ. No difference can be detected between the influence of the long and short swings. Combining the two we have as a result of 363 experiments a mean variation of 12^σ for those in the light, the same for those in the dark, and an average of 142^σ for the reaction-time in the dark and 147^σ for the reaction-time when looking at a swinging lamp.

It is important to discover if possible whether this disturbance is uniform throughout the ten experiments with the swinging light, or whether it is confined either, as in the earlier experiments, to the first reaction after the light was turned on or to those first few experiments where the eye follows the moving light. For this purpose the averages of the first, second and third experiments and so on of all the sixteen series were calculated and found to be, before the light was turned on, (1) 145^σ, (2) 143^σ, (3) 144^σ, (4) 145^σ, (5) 142^σ, (6)

139^{σ}, (7) 144^{σ}, (8) 144^{σ}, (9) 135^{σ}, (10) 140^{σ}; after the light was turned on, (1) 154^{σ}, (2) 149^{σ}, (3) 148^{σ}, (4) 142^{σ}, (5) 144^{σ}, (6) 151^{σ}, (7) 151^{σ}, (8) 139^{σ}, (9) 140^{σ}, (10) 143^{σ}. This shows conclusively that the chief disturbance is found in the three reactions after the light is turned on. The greatest lengthening is in the first reaction. Part of that is certainly due to the same effect which was noticed in table IV, namely to the distracting effect of having the light suddenly appear. But the presence of a similar though less effect in the next two averages shows a distinct influence from the moving light after the first surprise is over. Therefore we infer that part of the lengthening in the first reaction with the swinging light is also due to the moving light.

Later on in the series there is one average which is nearly equal to the first one with the light but an examination of the individual experiments shows that this is due to the accidental presence in that average of four unusually long times.

From these experiments we conclude that *the influence of light sensations upon the time of reaction to sound is comparatively small when the light is steady, but becomes very marked as soon as the light begins to move.*

A practical application of this fact suggests itself at once. There is no advantage for purposes of simple reaction in having the room dark. But it should not be lighted by a window, else moving shadows and changing intensities will affect the reactor. It must not be lighted by a lamp or gas jet for they would each, in the case of a small room at least, raise the temperature or, in case the room were ventilated by a current of air, be made to flicker and so distract the attention. But when the conditions of the experiment do not require a dark room there can be no objection to the presence of an incandescent lamp. On the contrary the person experimented upon will feel more at ease in a lighted than in a dark room and will find it much easier to make notes of any important points which occur to him during the experiments.

Experiments showing the influence of sensations of sound upon the time of simple reactions to sound.

The experiments thus far described have had to do with the influence of sensations of light on the time of reaction to sound. A similar set of experiments was conducted for the purpose of investigating the effect of sensations of sound on the time of reaction to

sound. In the first place the sound used was a steady tone produced by an electric tuning-fork vibrating 250 times a second. This tuning-fork was placed on a shelf before the transmitter in the recording-room and run by a dip battery. When the telephone circuit was closed this tone was sent to the reaction-room and, when the two receiving telephones were connected together, was heard in the same telephone in which the signal to react was heard. The moveable drum shown in the diagram makes no noise and, when the operator is careful and the rest of the building is quiet, no sounds reach the ear of the reactor through this telephone save those of the tuning-fork and the signal to react. The tuning-fork sound is so loud that none of the fainter sounds in the building or on the street can be heard. This method could not be used with the electric drum on account of the noise of the motor, except by placing the transmitter and the fork in another room.

The method of experiment was similar to that employed in investigating the influence of light-sensations. Ten experiments were taken in silence, then the telephone circuit was closed and during the remainder of the series the person heard the steady tone of the tuning-fork in the same ear in which he heard the stimulus. In successive series the order of silence and sound was changed to eliminate the influence of sequence. After eight series had been taken the method was slightly varied. It was evident that as in the case of the steady light the disturbance was very small. It was thought that possibly, as in the case of a steady light, the influence would be greatest upon the first reaction after the sound was turned on. Therefore to get the full effect of this influence, instead of taking ten experiments in silence and ten more with the tuning-fork sound in consciousness, one experiment would be taken in silence, then the tuning-fork sound turned on for the second reaction, while the third would be in silence and so on through the series. In each case, however, the tuning-fork was turned on immediately after the reaction in silence, thus giving about ten seconds for accommodation before the signal to react.

By changing the intensity of the telephone-current the loudness of the tuning-fork sound was varied at will. For the purposes of these experiments it was kept as nearly as possible equal to that of the sound which served as the signal to react.

Table V gives the averages of all these series. In it no difference can be detected between the time of reactions in silence and those in which the tuning-fork was heard. Seven of the individual series

from which this table was compiled showed a slight difference in favor of silence and nine showed about the same difference in favor of those with the sound. Of those where silence and sound alternate the same thing is true. Four were longer in silence and five in sound. The final average gives a time of 153^σ and a mean variation of 19^σ for those in silence and a time of 152^σ with a mean variation of 18^σ for those with sound. The average for the writer was 141^σ for silence with a mean variation of 15^σ and for sound 139^σ with a mean variation of 16^σ, showing a slight difference in the same direction as the combined results of the three persons.

TABLE V.

	Silence	MV	*n*	250 fork	MV	*n*
C. B. B.	141	15	104	139	16	113
W. I. C.	167	26	33	174	22	33
A. F.	232	42	10	242	39	8
Weighted mean	153	19	147	152	18	154

When it was found that the presence in consciousness of a steady tone produced no appreciable effect upon the reaction-time, with the skill in reacting thus far obtained, it was thought best to try the influence of an intermittent sound, as in the former experiments a moving light took the place of a steady one. The most convenient instrument which suggested itself for this purpose was the metronome. SWIFT[1] investigated the effect of the ticking of the metronome upon discrimination-time and found different results according to the rapidity with which the metronome ticked. He also found that it lengthened the time of simple muscular reaction. In a series of 100 experiments he found the reaction to be 103^σ, with a mean variation of 9^σ, whereas 100 experiments taken while a metronome was ticking in the room gave 122^σ, with a mean variation of 12^σ.

During the first few experiments the metronome was placed in the reaction-room and so arranged that the pendulum was held to one

[1] SWIFT, *Disturbance of the attention during simple mental processes*, Am. Jour. Psych. 1892 V 8.

side by the electro-magnet until the circuit of the magnet was broken in the recording-room. Thus the metronome could be started at will during the series. But in the small reaction-room the sound was almost too loud to be endured. It was found more satisfactory to place the metronome on the shelf before the transmitter in the recording-room, as had been done with the tuning-fork. Here the operator could stop the sound as well as start it and could regulate the intensity so that it should be about the same as that of the stimulus. There was an advantage also in having the two sounds come in exactly the same direction.

The order was the same as before, ten experiments in silence, then ten with the sound and vice versa. In different series the metronome ticked 40, 80, 120, 160 and 200 times a minute. Table VI gives the averages for each of the rates.

TABLE VI.

Rate of Metronome	0	40	80	120	160	200
Reaction-time	152	156	184	186	179	169
Mean variation	13	17	26	20	20	25
Increase	0	6	32	34	27	17
Number of experiments	147	28	54	93	61	42

In the first experiments where the metronome was in the reaction-room, there was a great innervation and strain in all the muscles of the body called forth to withstand the influence of the metronome. This was not noticed while the metronome was ticking, but the moment it stopped the sudden relaxation was very evident.

At the beginning of the experiments there is a marked change due to getting acquainted with the sound but, owing to the short duration of the experiment, that influence does not continue after the first surprise has worn away. Judging from the complete inattention to a clock ticking in my room it is probable that after listening to a metronome tick for a few hours it would no longer affect the reaction-time. The stopping of the metronome might then have a temporary effect. Indeed, in these experiments the reactor soon be-

came so far accustomed to the sound that an occasional vibration of the metronome spring, just loud enough to be heard, seemed more distracting than the metronome itself.

From these experiments we conclude that *the influence of a sound-sensation upon the time of reaction to sound-sensation is very small as long as that sound is a steady tone but becomes very marked when the sound is intermittent.*

EXPERIMENTS SHOWING THE DIFFERENCE IN THE TIME OF REACTION TO SOUNDS WHEN THE SOUND IS HEARD IN TWO EARS INSTEAD OF ONE.

It has already been suggested that the best way to produce a sound-stimulus for purposes of reaction is to use a head-telephone with a receiver at each ear. The following set of experiments were taken to show the relation between the time of reactions when the stimulus is heard in one ear and the time of those in which the same stimulus is heard in both ears.

The arrangement of the telephones has already been described. The method of experiment was similar to that of the experiments which have been described above. The person reacted ten times with a telephone at only one ear, then without interrupting the series he placed the second telephone at the other ear and continued to react ten times more. To eliminate other influences the order was sometimes reversed and the first half of the series taken with the sound in both ears.

Table VII gives the average of twenty-one series taken in this way. With the exception of three cases, where the person had no experience, the reaction to the sound in both ears is much shorter than that of the reaction to the same sound in one ear. The average of the thirteen series upon the writer, who was the only one having experience in reacting, with the exception of Dr. Scripture, give for one ear a time of 147^σ and a mean variation of 19^σ, for two ears a time of 133^σ with a mean variation of 19^σ. The number of experiments with one ear was 108, with two ears 123. The average of eight series taken on four other persons gives for one ear a time of 207^σ with a mean variation of 38^σ, for two ears a time of 188^σ with a mean variation of 31^σ. The number of experiments with one ear was 83, with two ears 82.

A natural explanation of the difference between the reaction-time when the signal is heard in one ear and the reaction-time for the same signal heard in two ears would be found in the difference in

the intensity of the sound as heard in the two ways. In order to see whether this explanation was satisfactory or not, fifteen series of experiments were taken in which the stimulus was varied in intensity from a loud to a weak sound.

This change of intensity was secured by introducing a resistance-box into the telephone circuit. The battery then in use consisted of twelve gravity elements. With the normal resistance of the circuit they gave a current of 0.5 amperes. With an additional resistance of 100 ohms the current was reduced to 0.08 amperes and the click made by making the circuit was a weak sound compared with that heard in the telephone when the normal current was used. Yet it was a sound which could be distinctly recognized. A switch was so arranged that by a turn of the lever the resistance box could be brought into the telephone circuit.

TABLE VII.

	One ear	MV	n	Two ears	MV	n
C. B. B.	147	19	108	133	19	123
E. W. S.	269	58	17	199	38	20
T. J. L.	182	35	39	175	26	33
W. I. C.	192	29	15	201	33	39
Weighted mean	170	27	179	158	24	215

The average for 132 experiments with the loud sound was 143^{σ}, with a mean variation of 13^{σ}. The average of 126 with the weak sound was 153^{σ}, with a mean variation of 16^{σ}. The difference between the loud and weak sound was 10^{σ}. That between one and two ears was 6^{σ}.

Attention might be called in passing to the fact that in this set of experiments four series were taken in which the succession of loud and weak intensities was irregular. The experimenter was told to make it as irregular as possible. The average of these four series for the loud sound is 143^{σ}, for the weak sound 155^{σ}. The mean variation from the average of the whole set is, for the loud 16^{σ}, for the

weak 17^{σ}. The number of reactions to the loud sound was 64 to the weak sound 66.

These figures are in marked contrast to those of Wundt. With him 18 successive reactions to a strong sound gave a time of 116^{σ} with a mean variation of 10^{σ}. A set of 9 reactions to a weak sound gave a time of 127^{σ} with a mean variation of 12^{σ}. When the succession of loud and weak sounds was irregular the time for the loud sound was 189^{σ}, mean variation 38^{σ}, number of experiments 9. The time for the weak sound was 298^{σ}, mean variation 76^{σ}, number of experiments 15. The increase due to irregularity in his case amounts to 114^{σ}, in our case to 12^{σ}.

It can scarcely be that there was a greater difference between the sounds he used; the difference in the present case was so great that the loud sound, when the order was unknown, was greatly dreaded and always produced a decided shock.

There are two other series of experiments which are worthy of note, having been taken without warnings. Their average for forty experiments is 139^{σ}, mean variation 26^{σ}, whereas the average of the thirteen series taken on the same person under the same conditions but with the warning was 140^{σ} with a mean variation of 19^{σ}; total number of experiments 231. Wundt's figures on the same point are, with warning 125^{σ}, without warning 259^{σ}, total number of experiments, 61.

We have shown that a large part of the difference between the time of one and two ear reactions is due to the difference in intensity. But to test this point more closely another set of experiments was taken in which this factor was entirely eliminated, yes, more than eliminated. The reaction-time of one and two ears was compared as in the last set but at the same time the intensities of the currents were varied so that the sound heard in one ear was judged to be much louder than the combined result of a weaker sound heard in both ears. If intensity be the only factor in producing the difference between one and two ear reactions then the reaction under these conditions ought to be much longer for one ear than for two. In only two series was there any marked difference between the two. One of these is in favor of the loud sound in one ear the other in favor of the weak sound in two ears. The difference between the final averages of the ten series is only 1^{σ}, practically 0.

Therefore: *the reaction-time to sounds heard in two ears seems to be shorter than for the same sound heard in one ear even after due allowance has been made for difference in intensity.*

In all of these experiments the sound in two ears was located in the upper interior part of the head. Turning the attention toward the sound resulted in rolling the eyes upward ; the sound seemed closer at hand and less effort was required for reaction than when the stimulus was heard in only one ear; the reaction also seemed more automatic, especially when the attention was turned to other things.

Conclusions from the three preceding sections.

The general results of these experiments in reaction-time can be summed up as follows:

1. The experiments did not indicate any difference in reaction-time produced by changing the color of the light present in the field of vision.
2. No difference was detected between the times of reactions in the dark and those made while looking at a stationary incandescent light of six candle power.
3. When this light was in motion the reaction-time was lengthened.
4. No difference was detected between the times of reactions in silence and those made while listening to the steady sound of a tuning-fork making 250 vibrations per second.
5. When the intermittent sound of a metronome was substituted for that of the fork, the reaction-time was lengthened.
6. The reaction-time to a sound heard in both ears is shorter than when the sound is heard only in one ear, even after making allowance for the difference in intensity.

Introspective observations on reactions.

During the larger part of the experiments pencil and paper were kept in the reaction-room and immediately after each series of experiments the person reacting noted down any conditions liable to affect the time of his reaction and any observations which might throw light on the nature of reaction-time. These notes were afterwards transferred to the record blanks just below the records to which they referred in order that these records might be used more intelligently.

A careful study of the notes together with the records to which they refer brings out many interesting points. Not all of them can be touched upon here but the most important ones will be found below substantially as they were written down from day to day.

After a few weeks practice reacting becomes so much a matter of habit, that trying to recall what has taken place during a series is like trying to remember dreams. The more striking points are easily retained but the larger part of the points which are noticed during the series are gone beyond recall unless they are noted down within a few minutes. A little practice enables one to record all mistakes in reacting, all reactions to warnings, all reactions registered before the signal is heard and all cases where the reaction-time is greatly lengthened by a physical or mental disturbance.

NOTES MADE IN THE REACTION-ROOM IMMEDIATELY AFTER EXPERIMENTS.

The following abbreviations are used:

W=Warning.
R=Reaction.
M=Muscular.
Att=Attention.
"fore"=Reaction before the signal.
Met=Metronome.
Om=Omitted.
a=first, b second half of a series.

The figures are taken from the original records for comparison with the notes. The numbers of three figures are reaction-times, those of one or two figures are mean variations. Where two sets of figures are given the first pair is the first half and the second pair the second half of a series. When the note refers to a particular experiment that one is compared with the rest of the series and its figures are given first. In order to make the meaning of the notes clearer explanatory remarks have been added; these are distinguished by italics.

1. R. to light.
2. Sounds heard outside.
63. Irregularity due to novelty of M. (134-6—138-17). *Showing that the irregularity was over-estimated.*
65. Turn Att.=turn eye.
76. 6, 8-11. React with jerk. (133—135-8). *The average of these four is 2$^{\sigma}$ shorter than that of the rest of the series.*
78. React with full arm movement. (138-18—137-28).
79. R. to W.
82-4. Tired, nervous, slow. (134-26—118-20.) *Mistake in judgment.*
94. W. I. C. "No difference." (209-22—181-31.) *Showing lack of practice.*
95. Notice regularity with two ears. *The figures don't show it.*
96. a. 1. React in spite of resolve. (222—197-26.) *A purely automatic reaction, extra long.*
96. Trying to think of Schopenhauer. (200-27—179-24.) *At least 50$^{\sigma}$ longer than the normal reaction-time.*
99. One R. before will impulse.
98. R. to W.—b, 1, om. in relaxation of silence. Sound terrific. (Silence 133-7 —with sound 133-10.) *No effect from this very loud tuning-fork sound.*

100. a, usual way, b, tense muscles. (146–33—120–16.) In one case will didn't overcome pressure. Att. confused in learning a new lesson. *In spite of confusion the reaction with tense muscles are 26σ shorter than the others!*

105. Last four, extra effort. (168—161–10.)

106, 7. Dr. Scripture "Not used to react without W. Have to 'wake up' when only one ear is used." *Two-ear reaction is more automatic.*

108. R. to small sound raises finger but little. *Showing the reflex element in all reactions.*

109. a, 5. R. too weak to raise finger, b, 3 "fore." 7–10, absorbed in plans.

111. a, one "fore." All sorts of distractions, pain in toe, wagon, shadows, thought.

113. Last one quick. Way prepared for motor impulse. (127—156–15.) *Correct estimate.*

114. 9 "fore." Met. very loud, scarcely hear W.

115. R. to W. Last 3 good. First 2 om.

116. Met. gives terrific sound.

119–21. Effort to touch a point 6 in. away.

126. The same. Very quiet; perfect type. b, att. more to muscles. (133–13—139–11.)

128. a, one ear, b, two ears. Two slightly longer. (148–13—142–23.) *Error in judgment.*

127. One om. driven out by another idea. R. to W. Every W. heard with tendency to R.

129. Sensory—M. M. quicker, harder. (129–10—127–9.) *Not enough quicker to be detected.*

130. a., ½. Terrific scraping; = insulation worn off in key. b, Att. = rolling eyes up.

134. 2–1 ear. 2 ear quicker. (127–7—140–15.) *Correct.* a, 9 Automatic. Mind returning from wandering surprised. (124–7—133.)

136. Att. all over, affects nerve force.

137. Loud—faint. Tend to give more Att. to faint sound at first.

141. Att. wandered. (157–17.) *20σ longer than normal.*

142. (153–14—132–9.) } Exercise between 142 and 143. Quick pulse, deep
143. (129–6—127–13.) } breathing. *Shortening the time 24σ.*

144. Careful Att. Sensory (?). Eye turned up. a, 5, long (202). b. 6, long (226). (153–18—170–7).

145. Faint demands, loud compels Att. Att. wandered. (Faint 152–17—loud 132–4).

149. 9, "fore." M. Faint-loud. Don't notice dif. intensity. M = hard work. (150–15—156–31.) *Attempt at muscular reaction a failure.*

150. React best way. Att. good. (168–19—171–8.) *Error in judgment.*

153. Faint-Loud. Irreg. No accommodation, no guessing. (148–16—167–21.)

154. No influence from knowledge of problem. One R. too faint to be recorded.

156. Sound varies from large sound to small short sharp pops = poor contact in multiple-key.

158. 1 ear loud—2 ear week. b, 1 No R.; too weak for one ear.

159. b, 1, loud sound in two ears. Awful start—Quick. (128—141–7.) *A case where the reflex element shortens the time.*

160. Last half thinking of something else. (134–159.)
161. Met. magnet didn't work. Started it myself.
164. Distr. of small sounds more than Met.
171. Reverberation of Met. spring very distr. (160–12—196–21).
172. Head most aches. Not feeling well. W. don't nerve me up. Sleepy. (132–11—156–16).
176. One "fore."
177. Sleepy. Not energy enough to tell what I have done. It was pretty good. (156–17—193–29.) *Error in judgment.*
178. Not so good. Uncomfortable position. Thought distr. more than noise. (172-27—167-13.)
182. Clear mind, like crystal, a sharp frosty morning, or the clear blue sky. (149–16—155–19).
186. Thinking of key. Does it distract? (150-21—196-48). *Decidedly.*
184. Noise of met. board provokes me. (151–8—205–9.) *Showing that irreg. sounds are much more distracting than the metronome.*
195. Two om.; one slow. No light at first. 2–4, one ear. Wagon: Out of patience! (136–8—158–15.)
196. Best yet. (154–12—129–8.) a, 1–6, uneasy. b, first rate; key between fingers.
198. a. Not esp. good. (134–16—146–10.) 2–4, Att. off. 147, 138, 139. *As good as 196.*
204. Some one up stairs. Warning out of order. (151–13—158–12.)
201. Distr. small. (130–7—139–10.)
202. Too fast. Fairly good. (127-10—134–17.) Last 3 thinking of spark. 125, 130, 128.
205. a, 6. Wagon heard on the street. (122—136–13.) *No disturbance shown*— b, 3. Seashore dropped something. (156—139–12.)
208. Seems long. Att. Wanders. (159–15—161–14.) *Decided effect.*
209. Little Att. Not tired. No ability to apply myself. (171–10—151–6.)
206. First rate, last half of b, distr. by wagon because I ought not to hear it. (139–16—156–13.)
207. Not so good. Too excited. b, light steadies me. (143–9—148–17.)
208. Hard to hold att. for 20 experiments. (146–11—162–11.)
209. Impossible to do good work. Think of everything. (138–9—151–13.)
210. Old position, elbow on table. First class example of inattention. Reaction natural in the flow of ideas. (141–13—140–8.)
211. First rate; b, 1, "fore." Innervate finger and fore arm. (132–14—141–11.)
213. Very good. b, end, M. (130.) One before sensory. (129.) Perfectly passive. Not innervation enough to hold the key. (129–8—141-20.)
214. Not quite as good. (121-12—135–15.)
215. Try to get away from bodily feelings=innervate Cortex. (128-7—147-13.)
216. The same. (126–12—145–9.)

The first impression on reading over these notes is one of surprise at the number of experiments in which there is some disturbance of the attention aside from that of the stimulus and warning. No amount of care in the preparation of a reaction-room and in remov-

ing external influences can do away with skin sensations, with muscular feelings, with changes in the physical and mental condition or with the ceaseless flow of thought.

An examination of the notes shows that these disturbances vary greatly. At times they are very prominent; then again they will be scarcely noticed. Record 196 is such a case. The note says of the series as a whole "best yet," of the first half "uneasy" of the second half "first rate." The figures are: dark 154^{σ} with mean variation 12^{σ}, steady light 129^{σ} with mean variation 8^{σ}. Records 134–7 also ought to have more weight than the average series. Their note says "best part of the day, excellent physical condition, mind clear and sharp." The figures in this case are: two ears 129^{σ} M. V. 9^{σ}, one ear 139^{σ} M. V. 14^{σ}. Certainly the average of a dozen series all of which had a similar certificate of the conditions under which they were taken would give different results from those of a set made up from records like 178. The note on this record reads "Not so good; uncomfortable position. Thought distraction very great." The times are: silence 167^{σ} M.V. 13^{σ}; metronome 172^{σ} M.V. 27^{σ}. To gain the most trustworthy results a large number of experiments should be taken and only those used which are free from all conscious disturbance.

Though introspection is of great value in estimating the general conditions of an experiment and showing the influences which affect the results yet it is not to be trusted in estimating the results themselves. Aside from the fact that the mind is unable to accurately estimate small divisions of time under the most favorable conditions,[1] its judgment is peculiarly liable to be affected by the conditions of the reaction. Its report is what it thinks ought to be rather than what it actually sees. For instance, in series 82–84 the reactor was tired and nervous and therefore judged the reaction-time to be longer than usual. The figures are: one ear 134^{σ} M.V. 26^{σ}, two ears 118^{σ} M. V. 20^{σ}, showing that the nervous excitement more than counterbalanced the physical fatigue.

In series 150 after having reacted in the muscular way in the previous experiment, the person reacted in the way which he thought would give the shortest time. Accordingly he judged the time of that series to be less than that of the preceding one. The figures for this one are: loud 168^{σ} M. V. 19^{σ}, weak 171^{σ} M.V. 8^{σ}; for the preceding one: loud 150^{σ} M.V. 15^{σ}, weak 156^{σ} M.V. 31^{σ}, being just the reverse of his judgment.

[1] MARTIUS, *Ueber die musculäre Reaction und die Aufmerksamkeit*, Phil. Stud. 1891 VII 167.

The last four reactions of series 76 were made with a violent jerk. More effort was put forth and the mind inferred that the reaction must be quicker. The average of the last four was 133^{σ}, that of the first half of the series 132^{σ}.

Again, in 78 the reaction-time was judged shorter than usual. In this case the hand was raised to the shoulder in every reaction. There was more motion and more effort, therefore the mind judged that the reaction started quicker. The figures are : silence 138^{σ} M.V. 13^{σ} ; with fork sounding in the telephone 137^{σ} M.V. 28^{σ}.

In several series, 126a, 126b, 127 the reaction consisted in touching a point on the table six inches from the key. Raising the finger from the key to make this motion broke the spark-coil circuit, and so only the beginning of the motion was registered. Here again the mind was mistaken in judging that the reaction-time was quicker than usual. The average of the parts of these series not subject to other disturbing influences was 141^{σ} M.V. 11^{σ}.

In connection with these experiments where the attention was directed to a motion for which the reaction was a means, the idea suggested itself, but has not yet been carried out, of having a second reaction-key in place of the point on the table. Then we should have recorded in addition to the reaction-time, the time required to make a certain movement. This would doubtless vary from time to time with changing mental and physical conditions. None of its variations could be attributed to influences acting upon the conscious part of the reaction as it would be purely automatic after slight practice. This might throw some light on the relative portion of the variation in reaction-time which is to be assigned to the purely psychical part. Possibly it might be used instead of the simple reaction as a standard for comparing the different kinds of reaction-time.

Between 142 and 143 the reactor went through vigorous muscular exercise so that, whereas 142 was taken with the body in a quiet passive state, during 143 the pulse-beat was strong and rapid, the breathing deep and heavy and the whole system generally excited. The figures for 142 are : 153^{σ} M.V. 14^{σ} for a loud sound in one ear and 132^{σ} M.V. 9^{σ} for a weak sound in two ears. The figures for 143, after the exercise, are : 129^{σ} M.V. 6^{σ} for the loud sound in one ear and 127^{σ} M.V. 13^{σ} for the weak sound in two ears. The shortening of the time by exercise was 14^{σ}.

But the chief value of these notes is found in the light which they throw upon the nature of simple reactions. In the first place, are these reactions muscular or sensorial?

Wundt says that, with a signal loud enough to be clearly heard, we naturally react in the muscular way. He explains the difference between the time of reactions to weak sounds and those to loud sounds as due to a passing from the sensorial to the muscular way of reacting. In all of our experiments, with a few exceptions, the signal to react was a loud sound. This would seem to indicate that our reactions were muscular. Furthermore, the two criteria, which Wundt regards as sure signs of muscular reaction, were both present. In six cases at least a reaction was registered just before the signal to react was heard, while scarcely a series passed in which there was not one or more reactions to a warning.

On the other hand, throughout the whole course of experiments the reactor believed that he was reacting in the sensorial way. The attention, with the few exceptions about to be mentioned, was invariably directed to the ear or rather, as it seemed to him, to the sound to be heard. The eyes turned in that direction and there was a distinct feeling of accommodation in that part of the head, due to a combined muscular and nervous excitation. The reactor even went so far as to attempt reactions after the muscular way, carefully directing the attention toward the hand or the movement to be made. In one case, before the reaction habit had been formed, the reaction was shortened in this way from about 140^σ to 100^σ. In one of the later series, 139, an effort was made to have the last three reactions muscular, two of these were 110^σ and 112^σ while the average of the other half of the series was 142^σ M.V. 8^σ. These would seem to be true cases of muscular reaction, if there be such a thing. But in general the attempt to shorten the reaction-time by turning the attention toward the hand or the movement to be made was a decided failure. More often the time was lengthened. It seemed very difficult to overcome the habit of turning the attention toward the ear.

It would seem that those experiments in which the attention was directed to a peculiar movement of the hand or arm, to touching as quickly as possible some point near the key or raising the hand as quickly as possible to the shoulder, satisfied the definition of muscular reaction. Yet none of these show any signs of a decrease in the reaction-time.

Finally, in none of the reactions save those in which there was an attempt at the muscular method, did the person experience that peculiar physical fatigue which is generally ascribed to the muscular mode of reacting. The fatigue was always mental. As such it was very marked at the end of five or six series. From this point of view these reactions must be regarded as sensorial.

But again, according to WUNDT, the peculiar nature of muscular as opposed to sensorial reaction lies in the fact that, while the sensorial reaction contains an apperceptive link in the chain of causes leading to the reaction, this disappears in the muscular method and the reaction becomes a purely automatic brain reflex.

Turning to the notes taken in the reaction-room we read, "Reaction a, 1, series 96, was made in spite of a resolve not to react." This was certainly a purely "automatic brain reflex" and contained no "apperceptive" link. The time of this reaction in the table is 222^σ. The average of the rest of the series 197^σ M.V. 26^σ. Reaction a, 9, series 134, was made while consciousness was entirely absorbed in something else. When it returned to the scene of operations it was quite surprised to find that a stimulus had been received and a reaction made in its absence. Its servants had done better than it expected. Surely this must be an "automatic brain reflex," with no trace of conscious perception or "apperception." The time was 183^σ, that of the whole series 124^σ M.V. 7^σ.

In series 99 one reaction took place before the will to react. That was set down at the time as a brain reflex. Its time however was not materially different from that of the other reactions in the series.

Several times, for instance in 108, it was remarked that a reaction to a weak sound only raised the finger a little whereas the reaction to a loud sound raised it over an inch.

These instances all seem to show that as soon as the habit is formed all our reactions are in the main brain reflexes. But they show just as conclusively that quick reactions are something more. The reflex action left to itself is slow, compared with the reflex action with the concentrated effort of the mind to hurry it along.

WUNDT lays great emphasis upon reactions to the wrong signal as proving the reflex nature of muscular reaction. MARTIUS[1] takes him to task for this and proves that he is wrong by showing that the same phenomenon occurs in sensorial reactions. WUNDT is right. They do prove that muscular reactions are brain reflexes. But they also prove that sensorial reactions are reflex in exactly the same sense. A note was made, referring to 127a, in which there had been a reaction to a warning, "every warning is heard with a tendency to react." And so it is with all sounds. All sounds tend to produce motion in some part of the body. We notice it in the case of loud sounds, in the case of sounds which startle us and especially in the case of rhythmic musical sounds. In the same way reaction is a

[1] See the article cited above.

brain reflex. Therefore the difference between sensorial and muscular reactions is not to be found by deciding whether or not they are brain reflexes.

The key-note to much of the confusion about the different kinds of reaction lies in the indefinite use of the word attention and in a lack of careful introspective analysis of what actually takes place in consciousness.

The word attention is most commonly used in the sense of ideational attention. This kind of attention is described by such expressions as: "Having a clear idea of the object of attention;" "keeping the object in the foreground of consciousness;" "thinking about an object calmly and quietly, yet clearly." It is passive as distinguished from the two following varieties of attention. There is little feeling of effort. What there is, is largely devoted to the inhibition of other ideas. Doubtless there is a slight innervation and muscular contraction but they are not prominent in consciousness.

This kind of attention can be directed to any part of the body, to any motion to be made or stimulus to be perceived, or to something entirely disconnected from the experiment. For instance, in one series, 96, the reactor fixed his attention on a lecture which he had just attended. The time for one ear was 200^σ M.V. 27^σ, for two ears 179^σ M.V. 24^σ. This exceptionally long time was not due entirely to voluntary diversion of the attention; the series was also taken without a warning to tell when the signal was to be expected.

As a rule this kind of attention does not shorten reaction-time. So far as distinct thought forms are concerned, the nearer a blank the mind can be kept the more satisfactory seems the reaction.

Often, turning the attention to another object seems to facilitate the movement, just as in writing the hand moves more freely when the attention is directed to the word being written than when it is directed to the muscular effort involved. It seems to drain off the surplus ideational force and leave the field clear for the reaction.

Using the word in this sense MARTIUS is right in criticizing MÜNSTERBERG when he speaks of the idea of the sound as fusing with the idea of a movement to be made. There is no fusing together of these ideas. Association is the word which describes their relation to one another, and the association is always a temporal succession, so far as the ideas themselves are concerned. It is impossible to keep two of these ideas in consciousness for any length of time. When the effort is made, it results in a rapid passing from one to the

other. First one comes into the foreground of consciousness, then the other.

A second sense in which the word is used is that of neural attention. Were it not for the fact that the word usually implies an increase in the muscular tension this might be called innervation. It results in bringing into consciousness the neural sensations. Sometimes it seems like a voluntary control of the nerve-force similar to the involuntary change which is produced by a severe strain when an increase in nervous excitement for the time being counterbalances physical fatigue. Closely combined with it there may be more or less of the feeling of outgoing will force as experienced in willing-games where one person wills another to do some particular thing.

This is closely connected with a third meaning of the word attention, namely, feeling-attention, i. e. the becoming immediately conscious of different parts of the body, which may be expressed either as bringing those parts into consciousness or as extending consciousness into them.

Using the word attention in either of these meanings it is possible to turn the attention to different parts of the body at the same time and to speak of ideas melting together. But the ideas here are quite different from those which we found in the first division.

In one case, 136, this attention was directed to all parts of the body at the same time. It seemed to increase the nerve-force and to result in a general excitement of the whole system. The average time of this series was 148^σ M.V. 17^σ, that of the series just before and just after, under the same conditions was 129^σ M.V. 10^σ. But a single experiment is hardly sufficient to show the effect of this kind of attention. No experiments were taken in which this kind of attention was directed to one or two parts of the body during a whole series.

A fourth sense in which the word attention is used is that of muscular attention. Involuntary changes in the condition of the muscles take place in response to various psychical changes; for example, they become tense in a fit of anger. To a large extent a similar change may be produced by voluntary effort. Something similar to this is usually meant by the expression "innervate the muscles." We can keep the muscles of the hand or arm relaxed or in a state of tension independent of the three kinds of attention already mentioned. Record 100 is an example of this; the first half was taken in the usual way and had a time of 146^σ M.V. 33^σ, the second half was taken with tense muscles and had a time of 120^σ M.V. 16^σ. In

one of the experiments in this series the motor impulse for reaction was not strong enough to overcome the pressure on the key.

A fifth way in which attention can be directed is best expressed as a preparation of the path for the motor impulse from the brain. This consists of a moderate innervation of the whole nerve tract, a small muscular tension and an effort to get a clear idea of the sound and the movement to be made, resulting in a rapid passing of the thought from one to the other. The last experiment of series 113 was made in this way; the time was 127^σ, that of the whole half-series 156^σ M.V. 15^σ.

A sixth state of the attention, one which requires as much effort of a certain kind as any, is that of inattention. All ideational attention, all neural attention and muscular attention are withdrawn leaving the reaction mechanism as far as possible to work automatically. This reduces the circulation, the nervous and muscular tension and with that the whole energy of the system. The tension of the muscles becomes so weak that they sometimes fail to move the very light spring in the reaction-key. By this means many persons are able to put themselves to sleep in a short time. Series 210 was taken in this condition; the time was, in the dark 141^σ M.V. 13^σ with a steady light in the field of vision 140^σ M.V. 8^σ.

None of these six descriptions accurately state the condition of the attention in the majority of our experiments. The most prominent feature was always an expectant attention, a strain in the muscles of the ear and a waiting for the sound. There was no attempt to form an idea of what the sound was to be, but simply an effort to hear it as quickly as possible. At the same time there was an under-current of neural and muscular attention directed to the hand and arm. If it is proper to use the terms primary and secondary consciousness, the primary was engaged with the sound, the secondary with the motor apparatus.

Aside from these seven phases of attention numerous other combinations of the three elements are possible. The dividing line between the classes is not a sharp one. All three or any one can be given especial prominence. Certainly the expressions muscular reaction and sensory reaction are of themselves very indefinite and give no accurate descriptions of the distribution of psychic energy.

With these different kinds of attention in mind it would be impossible to agree with James when he says,[1] in speaking of Wundt's and Exner's experiments, "The preparation to react consists of

[1] Principles of Psychology, I 438.

nothing but the anticipatory imagination of what the impressions or the reactions are to be" and "It is impossible to read Wundt's and Exner's pages and not to interpret the 'Apperception' and 'Spannung' and other terms as equivalent to imagination." A little practice will enable one to become master of all the different phases of attention here mentioned and many others more difficult to describe.

The remarks thus far have referred to voluntary attention. On the other hand there is a large involuntary element always present in the state of attention at any given time. This changes with practice and with the mental and physical condition. One naturally falls into a habit of reaction. The attention in that case is largely involuntary and resists all efforts of attention in other directions but co-operates with additional voluntary attention in the same direction. When a person is sleepy he has little control over the attention. The same is true when he is wrought up to a high state of nervous excitement.

The mass of bodily feelings varies with changing physical conditions; when one is fresh and vigorous they are large and massive and the motor impulse to reaction seems to meet more or less resistance in passing through them. After prolonged bodily or nervous strain these feelings tend to grow less and their center gradually rises. The extremities drop out of consciousness and the reaction seems to take place with less resistance. The background of all these various forms of attention is the constant play of psychic life. Waves and ripples of feeling and conation are incessantly passing through the mind. More clear cut and easily recognized are the ideas and images of the memory or imagination which follow each other at their own fancy. Now they pass along lightly like fleecy clouds over a summer sky. At the sound of the warning they vanish from consciousness and all the energies of the mind are bent to catch the coming stimulus and make the reaction. At another time the thoughts roll along like heavy irresistible storm-clouds. They heed not the faint warning or even the signal itself. Throughout a whole series the mind will be busily engaged inventing new apparatus, improving the old or struggling with some great problem of life. The reaction, so far as consciousness is concerned, goes on automatically. No amount of effort at the end of the series can call into consciousness a single fact connected with the reaction, nothing save the elements of this dream-like stream of consciousness. But during such a series let there be a break in the regularity in the recurrence of warning or signal and the mind is instantly alert to

inquire into the matter. Though apparently absorbed in other matters, yet there is a kind of unconscious attention being directed to the reaction.

Again, when the mind is apparently wholly engaged in the matter of reacting, an idea will suddenly come into the mind from some mysterious quarter with force enough to carry away for the moment all the attention, unconscious as well as conscious. In series 127 there was such an instance. An idea suddenly flashed upon the mind just as the signal was heard and the motor impulse being started; the whole force of the attention was diverted and the only impulse which reached the hand was a tremor too weak to raise the finger.

These observations and others suggested by the summary of the notes taken during the course of the experiments show that there is still much which can be learned about the mind and its processes from the study of reaction-time. They suggest a large number of experiments which might be carried out along the different lines of attention. Most of all they emphasize the great value of introspection. For the rougher work in reaction-time this may not be necessary. We can easily detect differences in reaction-time due to the presence of very distracting influences, to the effect of practice, to changes in intensity of the stimulus, to a change in the quality of the stimulus, and to many similar changes. But as soon as we inquire into the nature of reaction, and try to make quantitative estimates of these changes and to learn more about the nature of the mind's activities, then introspection is necessary.

Some will criticise these reaction-experiments because they were all made upon one person, and therefore can give no idea of what would be the result in another case. They say that results to be of value must be made upon a great many persons and the average computed as has been done in the case of physical measurements.

In reply it may be said of physical measurements that while statistical results from one point of view are of great value yet all the important discoveries in physiology have been made upon individuals. Statistics would never have discovered the circulation of the blood or the constituent parts of the brain. The human body is for the most part the same the world over and a discovery in anatomy in one body holds good in all others. The same thing is true in psychology. Nearly all the results thus far obtained have been gained from the study of individuals. The human mind like the human body is for the most part the same in its workings everywhere and

a discovery in one mind will hold good for other minds. Though important results will be reached along statistical lines yet the greatest advance in the future as in the past will in all probability be made by discoveries in investigating individuals.

EXPERIMENTS SHOWING THE EFFECT OF CHANGES IN THE STATE OF ATTENTION UPON THE MAXIMUM RATE OF VOLUNTARY MOVEMENT.

In a series of experiments carried on at Clark University, DRESSLAR found that the time required to make 300 taps varies with different individuals and with a change in the mental and physical condition of the same individual. Increased central activity favored an increase in the rate of voluntary movement.[1] BRYAN continuing the investigations upon a large number of school children found that the rate of tapping for the different joints of the hand and arm varied with children of different ages in accordance with their physical and mental development.[2] Many interesting points were brought out as to the effect of fatigue and the relative development of the different joints. But with the apparatus which they used it was impossible to show the relation between the individual taps of the series. The most that could be done in that line was to detect a slight decrease in rapidity due to fatigue when more than 300 taps were taken at a time. Otherwise it was taken for granted that the rate was uniform throughout the series.

The following experiments were taken with the purpose of investigating this question of the uniformity of rate. Our reaction-time apparatus offered the best possible means for measuring the interval between each successive tap. The taps were made upon the reaction-key in the dark room, and recorded by the electric sparks on the smoked paper of the drum. About 300 taps could be recorded on one paper by turning the drum slowly. An electro-magnetic time-marker was placed beside the tuning-fork and connected with a pendulum which beat seconds. This enabled one to tell at a glance the number of taps in a second in any part of the curve while, without it, it was necessary, in order to find the number of taps in a second, to add up the successive intervals, given in thousandths of a second, until they amounted to a second, and so on throughout the series.

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[1] *Some influences which affect the rapidity of voluntary movements*, Am. Jour. Psych., 1891 IV 514.

[2] *On the development of voluntary motor ability*, Am. Jour. Psych., 1893 V 1.

As a matter of fact this longer process was gone through in the preparation of the curves described below because there was a slight irregularity in the successive spaces marked off by the pendulum, due to the fact that the mercury contact was not made exactly in the middle of the arc of oscillation.

A click on the bell was the signal to begin tapping and the instructions were to tap as rapidly as possible until a second click announced that the record was complete.

TABLE VIII.

No.	A	MV	MV′	No.	A	MV	MV′
1	189	15	12	11	189	11	9
2	184	9	9	12	144	11	8
3	185	7	7	13	147	14	8
4	130	7	6	14	147	14	7
5	130	4	4	15	138	7	6
6	133	6	7	16	143	9	5
7	130	9	7	17	114	15	6
8	133	4	4	18	148	19	5
9	138	6	3	19	143	16	5
10	138	6	4	20	139	10	5

No., number of group.
A, average time.

Table VIII gives a summary of the figures for one experiment. There were 180 taps, or rather intervals between successive taps, recorded in this experiment. They were collected into groups of nine for the purpose of showing the gradual changes in the rate during the series; instead of finding that the intervals are all the

same, we found a continual variation. During the 180 taps there were only four cases where successive intervals were recorded as the same. The intervals were collected into groups of nine for the purpose of showing the gradual changes in the rate during the series; only the averages of these successive groups are given in the table. The deviation of the individual intervals was computed in two ways. The first deviation-column represents the average deviation of each

TABLE IX.

Name	Date	A	MV	T	S
W. I. C.	Feb. 25.	133	9.3	7.5	39.9
"	Feb. 28.	147	9.5	6.8	44.1
"	"	145	9.	6.9	43.5
"	"	147	10.	6.9	44.1
"	"	138	9.	7.2	49.4
Average		142	9.3	7.1	44.2
C. B. B.	Mar. 24.	175	11.	5.7	52.5
Dresslar		117		8.5	35.3
"	Av. of 20 visitors	167		6.0	50.4

A, average time.
T, number of taps per second.
S, number of seconds for 300 taps.

interval of the group from the total average of the 180 intervals. The second column represents its deviation from the average of its own group of nine. The difference between the two is due to the gradual change in rate. Looking at the first column one would say that the irregularity of tapping increased very fast toward the end of the series but a glance at the second column shows that this was not the case. On the contrary there is a steady increase in the regularity of the intervals in the different groups but, owing to the

rapid increase in the intervals themselves, the deviation of the later groups from the total average is greater than that of the earlier ones.

Six of these experiments or sets of taps, were taken and they all show the same general results. The average for the successive groups of nine taps for the six experiments were 140^σ, 137^σ, 136^σ, 134^σ, 139^σ, 144^σ, 146^σ, 147^σ, 148^σ, 150^σ, 145^σ, 150^σ, 150^σ, 154^σ, 154^σ, 154^σ, 153^σ, 152^σ and 150^σ. These averages show an increase in the rapidity of tapping for the first four groups of nine. After that there is a gradual falling off for five groups then a slight recovery during two groups followed by a still greater slowing up for four groups and then another slight recovery. The average of the first set of groups is 140^σ, that of the nineteenth, which is the last to contain records from all the six experiments, is 150^σ.

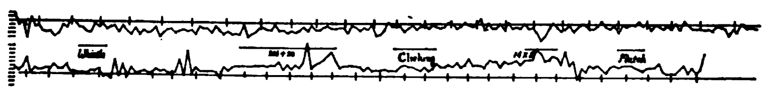

Fig. 14.

Table IX contains the final averages of the six experiments. Besides the average interval between successive taps, and the average deviation from it, the number of taps per second has been computed, and also the time required to make 300 taps. This was done for the purpose of comparing the results with those obtained by DRESSLAR and BRYAN. The time required for 300 taps is naturally somewhat less than it would have been if that number of taps had actually been made; for the experiments show that the last taps of a series are much slower than the first owing to fatigue.

However, we are not so much concerned with the number of taps in a series as with the regularity of the intervals between the successive taps. This can be studied much more satisfactorily by means of graphic representation than by tables. Accordingly the records of two series have been plotted. The resulting curves are given in fig. 14.

The curves represent the intervals between the successive taps of each series. The abscissas show the place of each tap in the series while the ordinates represent the length of the interval between two adjacent taps, each vertical division on the paper representing 10^σ and the horizontal line being taken to represent 150^σ. Thus the high parts of the curve correspond to slow and the low parts to rapid rates of tapping. The short vertical lines mark off the seconds.

A glance at these curves shows how the interval between the taps is constantly varying. Only in a very few instances does it remain the same for two successive intervals. For two, three or even four successive taps there will be a gradual increase in the rate until the mind is satisfied with its work or until it is unable to keep up this high strain of attention, when the rate again falls off. This effect can be seen in the second and third seconds of the first curve. Again as in the fourth and fifth seconds of this same series the process of change is more rapid. The rate alternately rises and falls with each successive tap. In the sixth second the rate falls off for three intervals, gains in the next what was lost in the last two, and again loses the same in the two following. Most of these changes pass unnoticed by the person while he is tapping. He is, however, at times conscious of a falling off in the concentration of attention and tries to correct it by a special effort.

The second curve, corresponding to the record of C. B. B. in table IX, is almost entirely above the 150^{σ} line, showing a slower rate of tapping than the other experiments. The explanation of this is the fact that the experimenter had learned of the great irregularity of the intervals and in this case aimed to tap as regularly as possible rather than as fast as possible. The result shows a slower rate but also the same irregularity, the same increase and falling off in the rate, now rapid, now gradual.

In all of these records aside from the usual variation in rate there are a few unusually long or short intervals. In the nature of the case there are more long ones than short ones. In one case there seems to have been a momentary break in the series, due very likely to a condition sometimes experienced by other persons in similar experiments. All of a sudden in the midst of a series of taps the arm seems to be momentarily paralyzed.

Over and above this variation in the rate from one tap to another there are larger gradual changes from second to second. During the first second there is always a general increase in the rate. After the middle of each curve there is a gradual slowing up. Dresslar detected signs of fatigue after 300 taps. My experiments show a decrease in the rate soon after 100 taps.

But aside from these two changes in the rate, an alternate increase and decrease of the successive intervals and a gradual slowing up toward the end, there is a third change brought out by these curves, which is of great interest. If we turn the curves around and look at them from the end so that the line of vision makes a small angle with

the plane of the paper we notice a gradual rise and fall of the curve every two or three seconds. This is doubtless due to the gradual rise and fall of the attention and corresponds to the alternate appearance and disappearance in consciousness of a sound just loud enough to be heard, as for example the ticking of a watch held at just the right distance, and with the alternate appearance and disappearance of faint rings on a rapidly rotating disk.[1] The chief point of difference is that in those cases the phenomenon is a matter of consciousness whereas in this case the person is entirely unconscious of the rise and fall in the rate. This fact shows that the rise and fall are not confined to ideational attention but are also characteristic of the subconscious muscular attention. These results agree with those recently obtained in the field of sight and hearing[2] in not showing any regular period of rise and fall. In general it occurs every three or four seconds.

In the second, third, fourth and fifth experiments an effort was made to distract the attention of the person tapping. The warning and signal used in reaction-experiments were sounded several times and the tuning-fork sound described in one of the experiments in reaction-time was turned on while the ear was at the telephone. In some cases there may have been slight changes in the rate of tapping owing to their influence, but that was by no means clear. In fact such weak sounds would hardly be expected to produce much disturbance in such heavy work.

In the sixth series the distractions were greater and show themselves plainly in the second curve. They were produced by an assistant in the reaction-room. A second electro-magnetic marker was placed on the drum and connected with another key in the reaction-room; by means of this key the assistant could register by the side of the time curve the beginning and the end of each disturbance.

The nature and place of the disturbances are noted just above the curve; their duration is indicated by the length of the short lines above the curves. In every case it will be seen that the disturbance produced marked changes in the rate. The blowing of a loud whistle was followed by a great irregularity. The mental addition of 214 and 23 at first made the rate very regular, more so than at any other point in all the series. This was during the period

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[1] LANGE, *Beiträge zur Theorie der sinnlichen Aufmerksamkeit und der activen Apperception*, Phil. Stud. 1888 IV 390.

[2] MARBE, *Die Schwankungen der Gesichtsempfindungen*, Phil. Stud. 1892 VIII 614.

between hearing the problem and beginning to solve it. At first the person was reluctant to undertake the problem. He felt that all his energies should be directed to the tapping but finally realizing that the problem must be solved he went at it. The first attempt at solution produced marked irregularly and real work was accompanied by a steady falling off in the rate. As soon as the answer was reached the attention returned to the tapping and the rate rapidly recovered.

A clicking with the tongue, such as is used to make a horse go faster, owing to this association seemed accompanied by irresistible impulse on the part of the person tapping to accelerate his movements. There was a slight falling off in the rate at the first surprise and then a gradual increase for four taps. But to the great surprise of the person who did the tapping, the rate did not exceed that which had been maintained since the last disturbance. The mental multiplication of 14 by 5 produced great irregularity as well as a general decrease of the rate. The sight of a lighted match, however, produced great regularity and a steady increase in the rate.

This sudden increase in the regularity of tapping without a marked change in the rate, when the attention is attracted by some other object, is similar to the fact noticed in some of the experiments in reaction-time, namely, that more regular results were sometimes obtained when the mind was partially absorbed in other things. The more superficial, ideational attention is directed to them while the unconscious muscular attention which is largely a matter of habit runs along more smoothly and automatically. As the mind is absorbed in the secondary problem, the more substantial subconscious attention gradually withdraws from the muscular effort and reinforces the mental effort.

On the other hand, as was also shown in the reaction experiments, some sudden surprise, in this case a clicking sound or a lighted match, at once draws away the deeper as well as the more superficial attention. But as soon as the surprise is over there is no strong intellectual effort required to watch the light and the subconscious attention returns unhindered to its habitual task, while the more fickle ideational attention remains captivated by the new sensation.

There can be no question that these last changes in the rate of tapping are due to disturbances of the attention. There can also be no question that the second change mentioned, namely the gradual slowing up after the first ten seconds, is due to fatigue. This

fatigue may be psychical, muscular or neural. Judging from the results obtained by LOMBARD in his investigation of the amount of work which can be done by a person under different conditions,[1] it is probable that the fatigue is in the nerve centres.

In the case of the change first described, namely the variation from tap to tap, the cause is not so evident. The fact that a partial withdrawal of the attention stops it and makes the intervals regular indicates a psychical cause. Under the additional strain of conscious voluntary attention the nerve mechanism acts more irregularly. Irregularity seems to be a characteristic of the higher forms of psychic life. The usual explanation is that there are two sets of nerve centres involved, the higher more unstable brain-centres and the lower more automatic ones of the smaller brain and spinal cord. A disturbance of the attention is supposed to cut off the higher centres from the circuit engaged in the muscular action. Yet both in the tapping and the reacting it was seen that further central activity was accompanied by further decline in the muscular rate. Therefore it seems proper to speak of a subconscious attention in this connection.

The explanation of the third change in the rate, namely the gradual rise and fall, is still more uncertain. Many persons will object to the use of the word attention in this connection. They would claim that it is a purely muscular phenomenon and regard it as supporting MÜNSTERBERG's explanation of the appearance and disappearance of faint visual images. The disappearance, he thinks, is due to fatigue of the muscles of the eye. As soon as the image disappears they relax and begin to recover; when they have regained their strength the object comes into consciousness again.[2]

It seems certain that this rise and fall in the rate must be closely connected with the appearance and disappearance in consciousness of faint sensations but it seems equally certain that they are not due to muscular fatigue. In the first place, it must be remembered that this rapid tapping is not a mere muscular operation; we have seen that the rate changes with changing psychic states. In the second place, there is no chance for the muscles to recover while the tapping continues. In the case of the eye there is a possibility that the muscles relax when the image disappears. Not so here. In the third place, the real fatigue shows itself in the general slowing up of the rate.

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[1] *Some of the influences which affect the power of voluntary muscular contractions*, Jour. Physiol. 1892 XIII Pts. 1 and 2.

[2] MÜNSTERBERG, *Beiträge zur experimentellen Psychologie* 1889 II 69.

Conclusions from the introspective observations.

1. Reaction-time is constantly affected by irregular disturbances a large part of which may be detected by introspection.
2. Introspection is not to be trusted in estimating results.
3. Exercise shortens reaction-time.
4. Reactions to the wrong signal, reactions before the signal is heard and the reflex nature of reactions are not sufficient criteria to distinguish muscular from sensorial reactions.
5. There are at least six distinct kinds of voluntary attention; ideational attention, neural attention, feeling attention, muscular attention, preparatory attention and inattention.
6. The involuntary attention is constantly changing.

Experiments showing the influence of disturbances of the attention upon the voluntary control of muscles.

There have been various devices invented to show the effect upon the body of various psychical disturbances. Some of the effects which have been pointed out are a rise in the temperature of the brain, a change in the circulation, a contraction of involuntary muscles, increased activity in the various glands, and a change in the force with which the muscles can be contracted. Lombard found that the knee-jerk showed marked changes in the case of mental disturbances. We have seen that the reaction-time and rate of tapping are influenced in a similar way. Jastrow[1] describes a piece of apparatus which he calls the automatograph, constructed for the purpose of registering involuntary movements of the hand. The hand is placed on a freely moving table to which there is attached a marker that records every movement upon a smoked paper. It was found that when the apparatus is screened from the eyes of the person experimented upon that the hand involuntarily follows in the direction toward which the attention is turned.

During the course of my experiments Dr. Scripture suggested that the accuracy with which a person could steadily point to a given spot would be a measure of the amount of attention he could direct toward the work. In accordance with that suggestion the apparatus shown in fig. 15 was arranged to measure this accuracy. It differed fundamentally from Jastrow's automatograph. In his case

[1] Jastrow, *Studies from the University of Wisconsin*, Am. Jour. Psych. 1891 IV 398; 1892 V 223.

the pointer was concealed from the person performing the experiment; here the pointer was in full view and every effort was made to keep it steadily opposite a given mark.

A receiving tambour was fixed to a standard in a horizontal position, face upward, so that the lever moved in a vertical plane. A light pointer eight inches long was attached to the lever. Back of the tip of this pointer a piece of card-board was fixed in a vertical position parallel to the plane of the lever. A dot was made on this card-board just below the end of the pointer. The recording tambour was adjusted to register the movements of this lever upon the smoked paper on the drum. An electro-magnetic time-marker was placed by the side of the recording tambour and connected with a pendulum which beat seconds.

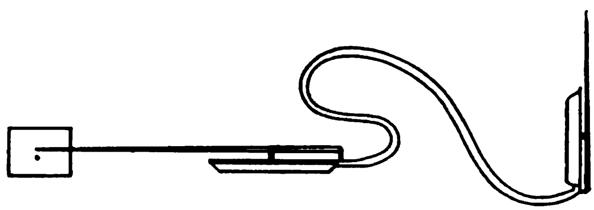

Fig. 15.

This apparatus having been arranged in a convenient position, a person placed his finger on the lever of the receiving tambour and, with the whole arm free, kept the end of the pointer as steadily as possible opposite the dot on the paper. It was found impossible to keep the point exactly opposite the dot; there was a constant vibration above and below. Within certain limits the movements of the point increased or decreased inversely with the amount of attention given to the work.

Figs. 16–19 contain sections cut from the record of one of these experiments. The upper curve was drawn by the lever of the registering tambour and shows the vertical movement of the finger. The heavy broken line was made by the time-marker and each section represents one second. The fact that some are longer than others shows that the drum was not turned with uniform speed; this fact must be kept in mind while examining the records.

The irregular shape of the curve shows the constant movement of the finger. In the centre of each of these sections there is a still greater irregularity. These disturbances all correspond to disturbances of the attention at the time the record was made.

A mark shows the point where the disturbance began, the white mark toward the right marks the point where the disturbance ceased.

Fig. 16.

Fig. 17.

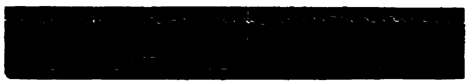

Fig. 18.

Fig. 19.

Fig. 16 shows the effect of an accidental distraction. There is great irregularity just at the time when another person happened to leave the room. The first half of fig. 17 indicates the effect of a sound which originated in another room. The distraction seemed to steady the hand ; great irregularity occurs about the time of return of attention. Fig. 18 shows the effect of the mental subtraction of 88 from 89. In this case the problem was so simple that the answer was given immediately, yet the disturbance is very marked. In fig. 19 the attention was drawn away from the work in hand by a person walking around the room.

These are only a few of the instances in this one experiment which show the inability of the mind to resist the smallest influences even when the will is set resolutely against them. It seems that in cases where the attention is distracted in a way such as to cause a tendency to move the eyes or when mental work is being done, the control of the muscles is uncertain. When the disturbance is a slight one, such as noticing a noise in another room, the distraction seems to aid the regularity. This latter case seems to be analogous to the well-known fact that we can perform numerous actions much better when only half attending to them.

ON MONOCULAR ACCOMMODATION-TIME

BY

C. E. SEASHORE.

Though the limits of accommodation and related problems have received due attention from Helmholtz and others, the *time* of changing the focus of the eye has hitherto been scarce investigated. VOLKMANN[1] seems to have been the first to experiment upon the subject. By applying Scheiner's experiment he found that he could change the accommodation of his practiced left eye 2? times from 11 in. to 6 in. and back in half a minute. VIERORDT[2] devoted a special treatise to the subject in 1857 and AEBY[3] later examined the duration of the act of accommodation within the limits of 315^{mm}. The latest and only important investigation on the subject that has come to the writer's notice is a series of experiments by BARRETT.[4] It will be referred to and compared with the present study from which it differs both as to method and results. The success of these experiments in Yale Psychological Laboratory is due to the efficient advice of Dr. E. W. Scripture, under whose supervision they have been conducted.

The problem I have undertaken is, to determine the time required to change the accommodation of the eye in either direction between two given points. As the object was the establishment of fundamental laws and not the collection of statistical material, this research is limited to observations on the right eye of persons with a normal eyesight accommodating for points in the direct line of vision under the most favorable circumstances. The psychological method of differential reaction-time has been used because it is apparently impossible to determine the accommodation-time by any direct physiological methods.

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[1] *Sehen*, Wagner's Handwörterbuch d. Physiol. 1850 III 1. Abth. 809.

[2] *Versuche über die Zeitverhältnisse des Accommodationsvorganges im Auge*, Arch. f. physiol. Heilkunde, n. F., 1857 I 17.

[3] *Die Accommodationsgeschwindigkeit d. menschlichen Auges*, Zt. f. rat. Med., III. Reihe, 1861 XI 300.

[4] *The velocity of accommodation*, Journ. of Physiol. 1885 VI 46.

Apparatus.

The variables are : (1) the distance of the nearer point, (2) the distance of the further point, (3) the direction of accommodation, i. e. whether the focus is to be changed from near to far or *vice versa.*

The solution of the problem required (1) an apparatus that holds the nearer point in view and suddenly exposes a point further off, (2) one that holds the further point in view and suddenly exposes the nearer point, (3) an arrangement to mark the instant the second point comes in view, (4) a reaction-key, (5) an apparatus for recording the time.

In the first experiments I used a revolving disc having holes near the edge through which the further point could be seen. The nearer point was indicated by objects on the disc at points alternating with the holes. The disc was arranged with weights, pulleys, springs and levers so that in its revolving it could be stopped to expose the nearer and the further point alternately. The person experimented upon was required to look through a tube extending from the eye nearly to the disc. One electric wire was connected with an isolated copper brush which made contact with the disc at the moment the second point could be seen. The brush was at all other points isolated from the metal plate by a cardboard covering. The other wire was connected with the disc and went through a closed-circuit reaction-key. Both wires were then run to the chronograph-room where they completed the circuit through an electromagnetic time-marker. Two markers were used on the drum. One registered the vibrations of the tuning-fork ; the other, running parallel to it, indicated the make and break of the current from the experiment room. To compare the two lines of the record perpendiculars were dropped from one line to the other at the points of make and break of the current.

After the first and second sets of experiments I used another apparatus which served the purpose better. With that I also used a simpler and more accurate method of recording. This latter apparatus consisted of a Laverne pneumatic camera-shutter to which electrical connections were added. There were two arrangements of the slide and the electric connections : (1) to drop the slide and expose the nearer point, (2) to raise the slide and expose further point. In the first arrangement one end of the electric wire was connected with the metallic body of the shutter. The other end was fastened to a binding-post which was connected (1) with a wire

spring which made contact with a projecting spring-arm on the slide at the moment the further point was cut off from view and the nearer point exposed, and (2) with a metal plate on which the projecting arm rested and made permanent contact when the slide came down. Both the contact-point and the metal plate were isolated from the metallic body of the shutter. In the second arrangement the slide was made to fly up and stop against a special catch. When the slide flew up its projecting arm struck the special contact spring at the moment the nearer point was removed from view and the further point was exposed. The special catch against which the slide finally rested at the top, and with which it made permanent contact was connected with the same binding post as the metal plate in the other arrangement. The current went through a closed-circuit reaction-key by means of which it could be interrupted. The current was made for an instant when the slide arm struck the special contact spring, permanently made by the slide arm resting on the metal plate or the special catch, and again interrupted by the reaction-key. The time required was that indicated between the first closing of the current and the breaking by the reaction-key. Heavy wires led from the shutter and the key to the chronograph-room where the circuit was completed.

In taking all except the first two sets of records I used the spark-coil method invented in this laboratory. For a full description of apparatus and method see the article by Bliss on p. 7–10. I did not, however, use the multiple-key described there but led the current directly to the research-room. A 100 v. d. tuning-fork was used but the drum was turned with such rapidity that the waves were sufficiently long to be easily estimated in tenths without error, thus giving me a direct record in thousandths of a second.

The nearer point was represented by a small capital o with a height of 0.7^{mm} and the further point by a large capital O with a height of 25^{mm}, except at the point represented as at infinite distance when a larger object had to be used. This object was a section in the crown of a distant chimney. The letter at the near point was on the slide. The other letter was on a card in a movable support.

The course of the investigation was somewhat in the following manner. First, the nearer point was kept constant at 20^{cm}, and the further points made respectively 50^{cm}, 1^{m}, 2^{m}, 4^{m}, 8^{m}, 12^{m}, and infinite distance. A point over 100^{m} away was considered the same as a point infinitely distant. Then the further point was kept constant

at infinite distance and the nearer point was made successively, 20^{cm}, 50^{cm}, 1^{m}, and 2^{m}. On these ten distances observations were made in accommodating the eye (1) from near to far and (2) from far to near. Both points lay in the line of direct vision. In this line was a tube extending from the front of the cornea in the eye of the subject to within a short distance of the nearer object. The tubes were adjusted at a sufficient distance from the slide to allow light to fall upon the nearer object. There was a special tube for each near distance. The 20^{cm} and 50^{cm} tubes had a bore of 2^{cm}. The 1^{m} and 2^{m} tubes had a bore of 4^{cm}.

Person experimented on.

These results aim to be the records of a typical case. I selected a subject who could well represent the average and gave him the most favorable circumstances, i. e. a comfortable position, good light, medium temperature and avoidance of anything that would distract attention. The results thus have uniformity and comparative value because they are taken on the same person and, as nearly as possible, under similar circumstances. The subject was an exceptionally critical and reliable observer—a fact of considerable importance where we have to trust to his judgment and faithfulness for the attainment of our results.

This series of observations was made on Mr. August Nelson, aged 29, a graduate student of philosophy. His eye is emmetropic with near-point 15^{cm} and far-point ∞; volitional ability, as exercised in concentrating attention, excellent. Reacting over 4000 times to the same stimulus he acquired considerable practice. Practice may have shortened his reaction-time slightly in the later experiments, but the variation was not great as he had made nearly a thousand reactions before I took the records upon which this article is based. Of other subjects reacting to the same stimulus, some take a longer and others a shorter time than N.; his records can be considered as representative of the average man.

Precautions and variations.

Reaction to a visual stimulus is a very complex act. It involves sensation, judgment, determination and other factors. In order to make the act as simple as possible precautions were taken to avoid intricacies and to facilitate perception. Thus, to avoid long and irregular time for discrimination and decision, the plain letter O was selected as the object for which to accommodate. The subject knew just what to expect and where to look for it.

The criticism was made during the investigation that it took time for the second object to "clear up." It was "blurred" at first and "gradually" became clear. That is just the point here investigated, viz: the time required to change the focus of the eye so as to make a clear image. The subject was instructed to use his best judgment and react when the object became clear to him.

Much of the mean variation in time on any one distance we may ascribe to the fluctuation of attention. Its effect upon reaction-time is to be considered as established. In this case I think attention was the great factor in determining the fluctuations around the mean time of "clearing up."

As a constant source of error I would mention the time that it took the slide to move a distance corresponding to the size of the letter at the nearer point. This time was less than 2^{σ}. Hence the uncertainty as to the time when one letter went out of view and the other was exposed cannot exceed $\pm 1^{\sigma}$.

Considering all the other objective sources of error, such as direction and strength of light, jarring of the apparatus and disturbances in the room, I would estimate that all the variations due to these do not exceed $\pm 5^{\sigma}$. The total limit of error was thus within $\pm 6^{\sigma}$. No uncorrected sources of error could be detected. In the earlier experiments where the unit of measurement was .01 sec. the latent time of the Deprez time-marker was quite negligible; in the later experiments with a unit of 1^{σ}, the latent time of the electric spark was far beyond negligibility, as was proved by experiments described in the article referred to above.

Method of experimenting.

The person experimented on was seated erect with the right eye before the tube. The left eye was closed but free to move. The reactions were made with the second finger of the right hand. The instructions were: "Look sharply at the first O until the second O is exposed; when you see the second O clearly, react."

The time of the operation indicated on the drum included the time of changing the condition of the accommodation plus the time of reacting to a given stimulus. To get the simple reaction-time I proceeded as above except that the subject was required to focus for the second object only, or on the place where it was supposed to be, and, without any change of accommodation to react every time he saw the same object again. To get the accommodation-time, i. e. the time of changing the adjustment of the eye between two focal

points, I subtracted the simple reaction-time. The records of the reactions were taken in sets of ten to twenty in each; hence, under similar circumstances. The first object was exposed about two seconds before the change and there was an interval of about ten seconds between each successive observation.

I added all the records on each distance in a set and took the average. The tables give these final averages with the mean variation and number of experiments corresponding to each.

RESULTS.

In the following tables I use these abbreviations: *N*, nearer point; *F*, further point; ∞, practically infinite distance; *F* ⟶ *N*, from far to near; *N* ⟶ *F*, from near to far; *n*, number of experiments of which the average is taken; *A*, accommodation-time; *R*, reaction-time; *AR*, accommodation-time plus reaction-time; *MV*, mean variation.

TABLE I.

(Curve I) *N* ⟶ *F*. $N=20^{cm}$. Unit of measurement, .01 sec.

F	*AR*	*MV*	*n*	*R*	*MV*	*n*	*A*
50^{cm}	27.2	7.4	39	26.5	3.8	18	0.7
1^{m}	28.4	3.1	40	26.5	4.4	16	1.9
2^{m}	34.8	4.4	37	32.2	3.7	13	2.6
4^{m}	36.1	2.6	31	32.3	4.0	19	3.8
8^{m}	40.9	7.6	37	35.5	6.3	20	5.4
12^{m}	41.4	11.2	40	31.9	5.7	20	9 5
∞	40.9	5.9	40	31.5	4.5	20	9.4

The figures in tables I and II express hundredths of a second, the last figure being simply the decimal obtained in averaging a column. All the other records are in thousandths of a second. For the sake of comparison and uniformity the first two curves are also marked

in thousandths of a second but it must be remembered that they are based upon the figures in the first two tables.

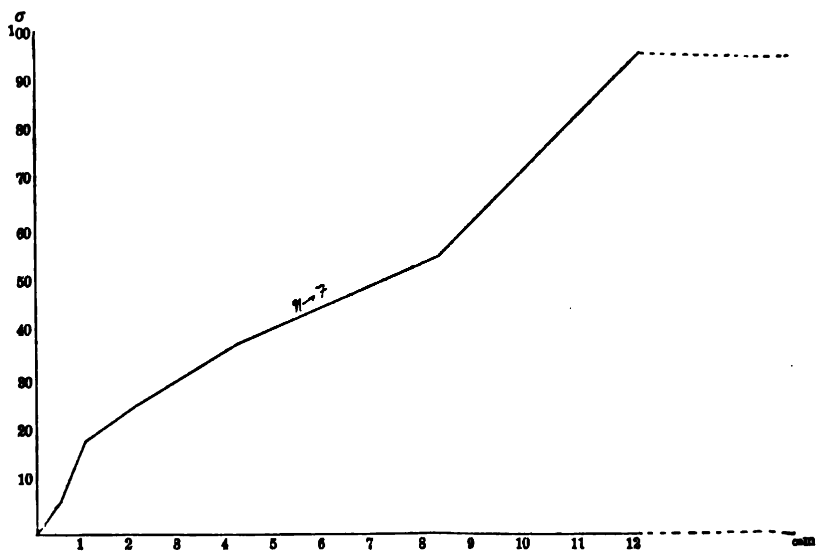

Fig. 20. Curve I.

This table shows that when N is constant, the time of changing the accommodation of the eye from N to F increases with the dis-

TABLE II.

(Curve II) N - - —> F. $F=\infty$. Unit of measurement, .01 sec.

N	*AR*	*MV*	*n*	*R*	*MV*	*n*	*A*
20^{cm}	40.9	5.9	40	31.5	4.5	20	9.4
50^{cm}	40.0	7.5	40	34.7	5.6	20	5.3
1^{m}	32.5	4.7	35	27.2	3.5	18	5.3
2^{m}	39.8	7.8	40	35.5	5.4	20	4.3

tance of F up to 12^{m}. Beyond 12^{m} the time of monocular accommodation does not vary because the rays are practically parallel for all such distances. It will be observed that in proportion to the distance of F the greatest change in time is when F is near N, and

that the ratio diminishes to infinity as the distance between the two points is increased.

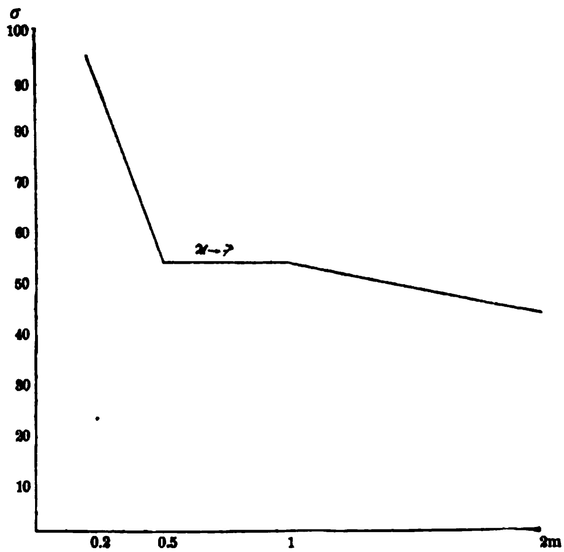

Fig. 21. Curve II.

When F is constant the time increases inversely with the distance of N from the eye up to 2^m or more. The greatest change in time

TABLE III.

(Curve III) $F \longrightarrow N$. $N=20^{cm}$. Unit of measurement, $\sigma=.001$ sec.

F	*AR*	*MV*	*n*	*R*	*MV*	*n*	*A*
50^{cm}	169	24	40	159	30	18	10
1^m	168	22	40	165	21	19	3
2^m	178	18	40	157	12	18	21
4^m	185	33	40	163	15	17	22
8^m	189	27	34	166	19	12	23
12^m	213	37	37	152	19	10	61
∞	250	29	34	206	19	20	44

is when N is near the eye. The law of relative variation is the same as in table I, i. e. within certain limits the accommodation-time varies with the distance between N and F, though this variation is not in proportion to the distance.

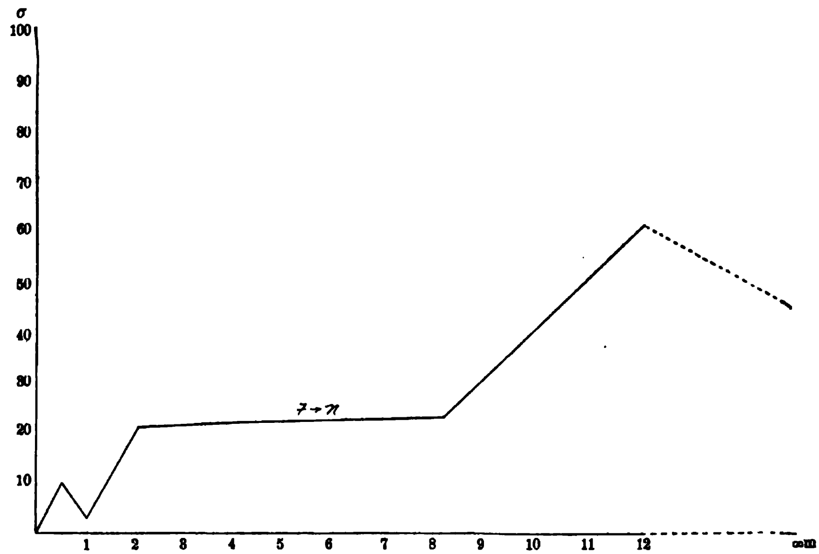

Fig. 22. Curve III.

The same general law is brought out here as in table I; but in this case ($F \longrightarrow N$) the time is shorter and the mean variation is

TABLE IV.

(Curve IV) $F \longrightarrow N$. $F = \infty$. Unit of measurement, $\sigma = .001$ sec.

N	*AR*	*MV*	*n*	*R*	*MV*	*n*	*A*
20^{cm}	250	29	36	206	19	20	44
50^{cm}	267	51	33	203	16	19	64
1^{m}	214	19	40	178	17	20	36
2^{m}	205	18	40	162	15	16	43

less than in the other ($N \longrightarrow F$). From a comparison of AR and R in this table it becomes evident that the deviation in the case

of the last figure is wholly due to an exceptionally long reaction-time.

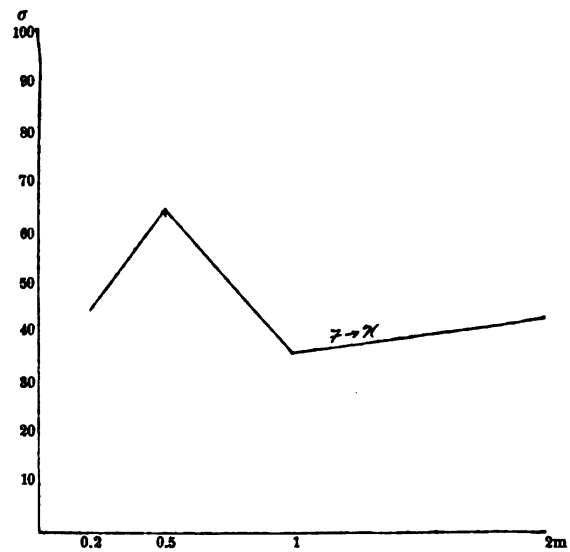

Fig. 23. Curve IV.

This is complementary to table III, and corroborates the same principles for the case where the nearer point is constant.

TABLE V.

(Curve V) *AR* taken $F \longrightarrow N$ and $N \longrightarrow F$ alternately. $N=20^{cm}$. Unit of measurement, $\sigma=.001$ sec.

F	a $F \longrightarrow N$	MV	n	b $N \longrightarrow F$	MV	n	$b-a$
1^m	174	18	40	183	13	28	9
2^m	184	37	39	220	18	36	36
4^m	211	35	38	229	22	38	18
8^m	269	44	38	297	71	35	28
12^m	335	65	34	398	105	29	63
∞	243	65	40	363	102	40	120

The purpose of this set of experiments is to adduce further proof for the facts brought out in tables I and III, and to show how the

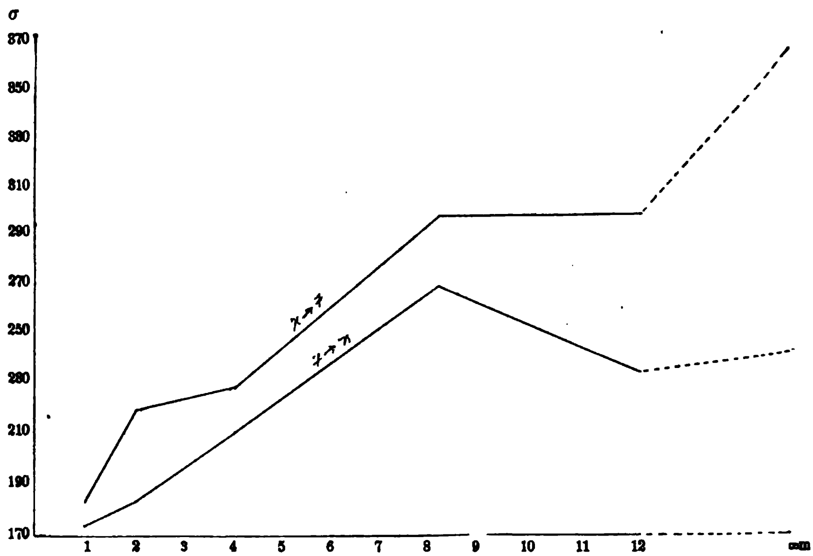

Fig. 24. Curve V.

time is influenced by the direction of accommodation. This table is a comparison of *AR* taken in series of 20 in each direction alternately.

TABLE VI.

(Curve VI) *AR* taken $F \longrightarrow N$ and $N \longrightarrow F$ alternately. $F = \infty$. Unit of measurement, $\sigma = .001$ sec.

N	a $F \rightarrow N$	MV	n	b $N \longrightarrow F$	MV	n	$b-a$
20cm	243	65	40	263	102	40	20
50cm	193	19	30	229	26	35	36
1m	208	28	27	222	29	30	14
2m	187	25	32	244	48	26	57

nately. It conforms to the principles laid down in tables I and III, and also shows that the difference between *AR* taken $F \longrightarrow N$ and

N ——> *F* varies with the distance of *F* up to 12^m when *N* is constant.

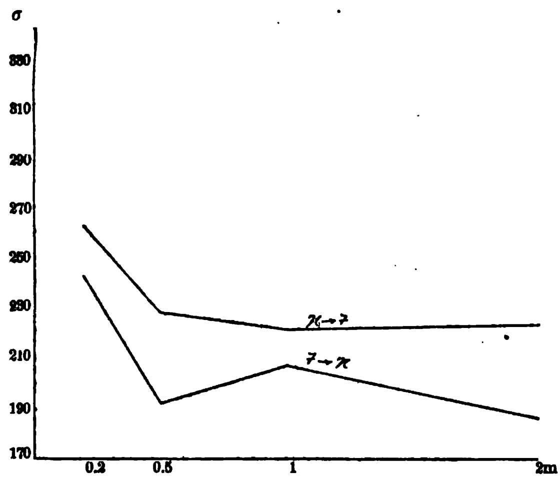

Fig. 25. Curve VI.

When *F* is constant, the difference between the *AR* taken *F* ——> *N* and the *AR* taken *N* ——> *F* varies inversely with the

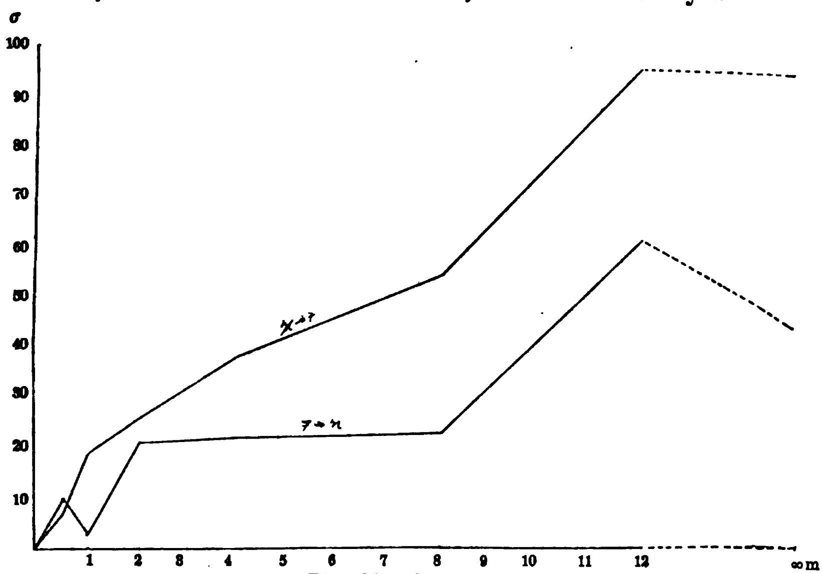

Fig. 26. Curve VII.

distance of *N* up to 2^m or more. This is complementary to table V, and confirms the same principles.

Curve V shows the difference in time depending on the direction of accommodation. This difference is further proved and illustrated by other data in curve VII, which is a comparison of the simple accommodation-time as given in tables and curves I and III. In the same manner curve VIII, complementary to curve VII, is a com-

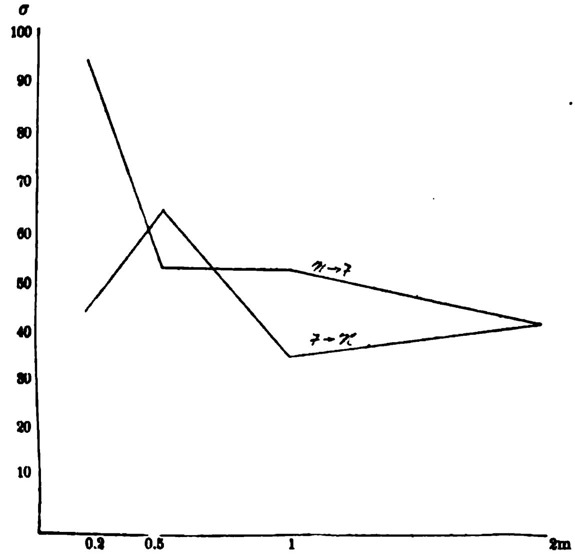

Fig. 27. Curve VIII.

parison of curves II and IV, and proves with A the same principle as curve VI proves with AR.

Aside from the time itself and minor conclusions which may be drawn from these figures, three important principles have been established.

(1) Within certain limits the accommodation-time varies with the distance between the points for which the eye is to be accommodated.

(2) It takes longer to change the accommodation from near to far than from far to near, and this difference in time varies directly with the length of the accommodation-time.

(3) For equal distances in the same range the accommodation-time is greatest for points near the eye and decreases with the distance of the points from the eye.

From the second fact arises the important question: what is it in the mechanism of the eye which will account for this difference in time depending on the direction of accommodation? It may be due

to the difference in time required for relaxation and contraction of the muscles controlling the lens, or to the difference in range of movement and sureness of adjustment in changing the form of the lens to focus for points at different distances from the eye. For a satisfactory answer to that question we must, however, look to further investigation on the physiology of accommodation.

Comparison with previous results.

Barrett's apparatus consisted of a skeleton compound microscope, supplied with electric connections by means of which the time was recorded on a drum. The experimenter carefully pointed out and obviated many errors of previous workers, yet his apparatus necessarily involved three sources of error which have been avoided by the apparatus in the Yale laboratory.

(1) Instead of using lenses, as Barrett did, the eye was required to look directly upon the object to be accommodated for.

(2) His apparatus measured accurately to 0.1 sec. only, while the present apparatus measures with accuracy within ± 0.006 sec. That his apparatus not only contained some great error but also gave very inaccurate records, can be seen from the frequent remarks, "time too short too be measured."

(3) The greatest error is, perhaps, due to his method of deducing the simple accommodation-time. The present method of observing the reaction-time (without any movement of accommodation) to the same stimulus, for each distance and under exactly the same circumstances is the only trustworthy plan. Barrett arbitrarily subtracted 0.4 sec. in all cases.

Additional records on six other persons, which have not been introduced into the tables in order not to disturb their value for comparison, agree with Barrett's conclusion, that accommodation-time varies according to different circumstances, of which the principal are (1) age, (2) practice, (3) individual characteristics and (4) time of day.

In regard to fatigue, however, my results are contrary to the usual supposition. Experiments of some 300 accommodations in one continuous set do not support that theory. Fatigue soon sets in and may become very painful, but as long as the eye can accommodate clearly it causes a fluctuation in time which tends more to accelerate than to retard the velocity of accommodation.

The most surprising deviation from previous results and theories is the conclusion drawn from my tables as to the relation of velocity in accommodation between near to far and far to near. Vierordt, Aeby and Barrett all agree that the accommodation-time is greater than the relaxation-time, i. e. that it takes longer to accommodate from far to near than from near to far. This statement is contradicted by every table and diagram given above.

ON THE RELATION OF THE REACTION-TIME TO VARIATIONS IN INTENSITY AND PITCH

BY

MORRIS D. SLATTERY, M.D.

METHOD OF EXPERIMENTING.

The plan followed was to place the person experimented on in an isolated room to which all stimuli were sent through wires from a distant room, and from which other sets of wires conducted the currents making electric registration in still another room.

The first requirement was thus a room free from disturbing lights and sounds; this was met by the construction of the isolated room. The isolated room is a small room built inside of another room; four springs of rubber and felt are the only points in which it comes in contact with the outer walls. The space between the walls is filled with sawdust as in an ice box. The room is thus proof against sound and light, and affords an opportunity of making more accurate experiments on the mental condition than yet attempted. This was the final construction of the room, adopted after numerous experiments. As such a room may prove valuable in physiological and medical work where freedom from disturbing sights and sounds is desired, it may be well to point out some of the difficulties to be overcome and erors to be avoided. The first requirement is a wall-surface impervious to sound. Owing to the great expense involved, such materials as asbestos, heavy tough hair-felt, lead, etc., could not be used; soft wood with outside packing of sawdust was finally chosen. The next requirement is that the sound waves from the building should not be transmitted to the frame work of the inner room, i. e. it must not be connected to the outer room. The nearest approach to the satisfaction of this requirement was made by supporting the inner room on pieces of soft rubber and avoiding all other connection with the walls of the outer room. The sound waves in the building are thus almost hindered from passing to the inner room. The vital importance of these precautions can be shown by simply laying a pencil across from the wall of the outer to that of the inner room; the various sounds are at once carried across and are heard in the inner room almost as loudly as if none of the other

precautions had been taken. The third requirement is the means of ventilation without sound conduction. After several trials a peculiar ventilator has been invented in which the air is made to pass back and forth through a tortuous passage, the walls and partitions in which are made of hair-felt. Sound waves can pass through bent tubes only by reflection from the walls and refraction around the angles; in the ventilator all reflection is killed by the non-elastic felt and the amount of sound transmitted by refraction through such a tortuous passage is so small that a person shouting into it at one end can barely be heard at the other.

The records were made by the graphic method, the usual chronoscope method being rejected as cumbersome and inaccurate. The counting of the fork-vibrations by the graphic method takes a somewhat longer time than the reading of the chronoscope records but in measurements of simple reaction-time this extra work is more than compensated by the saving on the laborious adjustment of the chronoscope and the necessary re-reckoning of the results. The new method of making graphic records, which was invented in the laboratory and used in the later experiments, not only greatly increased the accuracy of the records but made the work much less than that of taking chronoscope records. The apparatus used for recording was the same as that described by BLISS on pages 10–16.

In my investigations the multiple-key was connected with the isolated room and the apparatus in the following ways. The circuit from the tuning-fork through the time-marker was connected at one side to P and at the other to B, so that when P and B were in contact they short-circuited the current. This, of course, stopped all action of the marker while the contact was made, but as soon as the knob G was pressed the marker started. The circuit to the other marker was passed through the lever I to O and the platinum point, and out through Z and W, then to the reaction-room and through the reaction-key. As soon as the point of E touched U the lever I would break its contact at Z and immediately make it again at W, thus making a nick in the line of the second marker. As the stimulus circuit is closed at the moment E touches U this nick gives the moment of the stimulus. Since the current is immediately closed at W the marker is ready to make another nick as soon as the person reacting presses his key. The time-marker connected with the fork was set vibrating as soon as the knob G was touched, so that the curve was already being drawn before the record began. When the spark-coil is used the primary circuit is run over the same line as the

circuit used for the second marker ; the secondary circuit has its poles in the drum and in the lever of the marker connected with the fork. Any movement at Z or of the reactor's key, produces a spark. Three different arrangements had to be adopted for the three variations of the stimuli to be used. These will be described in the appropriate sections.

Experiments with different intensities of tone.

In all the experiments on tones they were produced by electric forks in a distant room. The sound thus produced was sent from the primary circuit of a telephone transmitter, which was placed near the fork, through wires to the multiple-key. The secondary circuit of the transmitter was connected directly with the reacting telephone in isolated room. In investigating the relation to intensity of tones a fork of 250 complete vibrations per second was placed in the distant room. For producing variations in the intensity, a resistance-box was placed in the primary circuit of the transmitter; by this means a resistance of 100 ohms could be introduced, giving a weak tone, or 50 ohms, giving a medium tone or zero, giving a loud tone.

The first person experimented on was E. W. Scripture. With everything in readiness for the experiments the person entered the isolated room, closed and fastened the door, and, holding the reacting telephone close to his ear, waited for the stimuli to which he would react by pressing on the knob of the reacting-key. Thirty experiments were made in series of 5 to 10 with each grade of intensity, the order being varied to eliminate influences of practice and fatigue. After each series an intermission of at least 5 minutes was given. Preceding the sending of each stimulus a warning was sent to the reactor to call his attention to the expected sound.

After the first evening's experiment, the person experimented on made the following statement. "Left arm resting, with telephone in hand close to ear. Loudest sound, quite loud. Reacting hand at rest except index-finger which is held upon key-knob. After the warning, attention was directed entirely to expected sound. Eyes were closed and while waiting for sound were turned strongly toward the left. General condition slightly fatigued."

The next person experimented on was D. O'Keefe, the experiments being performed in the same manner as the preceding ones. His statement after the experiments was, "Left arm resting with telephone in hand close to ear. Loudest tone quite loud, the weak-

est tone being just perceptible. Reacting hand at rest except middle finger which was held upon key-knob. After receiving the warning, attention was turned entirely to expected sound. Eyes open and turned toward the left. General condition good." Before making the statement he asked, "if there was any trouble with the apparatus as the tone changed in intensity at different intervals." He was not aware of the fact that the intensity was to be varied.

The results of the experiment are summed up in table I.

TABLE I.

Unit of measurement=1^σ= .001 second.

	S′	MV	*n*	S″	MV	*n*	S‴	MV	*n*
E. W. S.	282	68	14	287	61	17	299	72	15
D. O. K.	249	57	34	212	70	29	218	68	10

The first column gives the person experimented on, the second the reaction-time to the strongest tone, the third the mean variations from the average, the fourth the number of experiments, the fifth, sixth and seventh give the data for the medium tone, the eighth, ninth and tenth those for the weakest tone.

It will be seen from the results that for the loudest sound, in the case of E. W. S., we have a reaction-time of 282^σ; for the medium intensity the reaction-time was 287^σ, for the tone of weak intensity the reaction-time was 299^σ. The absolute differences for different intensities were small and were much less than the amount of the mean variation. We can therefore conclude that they are practically equal and that within the limits of intensity used in these experiments the reaction-time does not vary with the intensity to any degree that can be detected.

In the experiments on D. O. K. it will be seen that the differences in the reaction-time are reversed but are also small; with sound of greatest intensity the reaction-time was 249^σ, with the sound of medium intensity the reaction-time was 212^σ, with sound of weak intensity it was 218^σ. From these results the same conclusion is to be drawn as from the previons ones.

Similar experiments had been previously begun in March, 1892, by Dr. Scripture at Clark University on F. B. Dresslar, but were not carried to completion. The sound was produced by a secondary coil

connected with a telephone and placed over a primary coil through which passed a current interrupted by the vibrations of a 250 tuning-fork. The angle which the axes of the two coils made with each other regulated the intensity of the sound, which was in all cases weak. Five steps with coils at angles of 110°, 125°, 140° and 155° were taken. The records of March 22 were handed to me by Dr. Scripture to be used, if I saw fit, in comparison with my experiments. They were counted and the results will be found in table II.

TABLE II.

Unit of measurement=1^σ= .001 second.

S′	MV	*n*	S″	MV	*n*	S‴	MV	*n*	S⁗	MV	*n*
288	26	30	239	18	28	224	25	29	268	15	30

The columns refer to the same subjects as in the previous table, but four grades of intensity were used. The strongest sound is placed first. The reactor, Mr. Dresslar, said in a note at the end of the experiments, "that he perceived no difference in the intensity of the various sounds, i. e. to his consciousness the different sounds appeared to him to be of equal intensity." While actually there was a difference in the intensity of the tone, it must have been very slight. A comparison of the reaction-times of the four steps taken shows such a slight difference that the only conclusion to be drawn from the experiments is the same as in the previous case.

That other observers have obtained very long reaction-times for very weak noises might be explained by the fact that when the warning signal is used shortly before the stimulus is to be produced there is a natural tendency to revive an image of the sound in the mind. In the case of loud sounds the image would give rise to no confusion, but in the case of very weak sounds the observer might well be in doubt as to whether a sound apparently heard was such a memory or was actually produced by a stimulus. Some experiments on the border line between sensation and hallucination indicate such an explanation.

In the table of results obtained from experiments performed by WUNDT,[1] from which he concludes that the reaction-time decreases constantly with an increase in intensity of the stimulus, it will be seen that his figures do not bear out the statement. In his experiments the sound was produced by a ball falling from different

[1] Physiologische Psychologie, 2 ed., II 238.

heights; as the height increased, the noise became louder. His figures for successively louder sounds are 217^σ, 146^σ, 132^σ, 135^σ, 161^σ, 176^σ, 159^σ, 94^σ. It is at once apparent that with the exception of the weakest sounds, the results are in direct contradiction to the statement. It is but fair to add that these results are omitted from the later editions of the *Physiologische Psychologie.*

The experiments of MARTIUS[1] on tones and noises gave negative results, i. e., the reaction-times of the various experiments were nearly equal or showed slight and inconstant relations to each other. He drew the following conclusions: "There is no constant decrease in the reaction-time with an increase in the strength of the stimulus. Differences in the times occur only when very great differences in the strength of the stimuli exist, as for instance between a very weak and a very loud sound. The lengthening of the time with very weak stimuli can be accounted for by difficulty of perception."

My experiments on tones lead to these conclusions:

First—The law that the reaction-time decreases with increasing intensity of stimulus does not hold good for the sense of hearing, i. e. the reaction-time to tones is nearly the same for all moderate intensities.

Second—The longer time registered for very weak tones or noises by some observers is probably not due to any conscious change, but is caused by hesitation as to the actual hearing of the stimulus.

EXPERIMENTS WITH TONES OF DIFFERENT PITCH.

In using tones of different pitch the arrangement was nearly the same as in the previous case. Three forks were all kept ready so that in the few minutes of rest between the sets of experiments a change could be made from one to the other. The resistance was kept in the circuit and by preliminary trials the amount of resistance to be used for each tone was determined, so that all the tones seemed to be of the same intensity. This was quite necessary as it is impossible to adjust an electric fork so as to give the same intensity on different occasions. Similar precautions as to practice, fatigue, etc., were taken as in the previous case. The reactor seated in the isolated room heard the tones through the reacting telephone as before. The pitch of the tones was changed by changing the forks. Since the

[1] MARTIUS, *Ueber den Einfluss der Intensität der Reize auf die Reactionszeit der Klänge*, Phil. Stud. 1891 VII 469.

tones were adjusted so as to be of the same apparent intensity it would naturally be expected from the experiments of the preceding section that the reaction-times would be the same. The actual results are given in the following table :

TABLE III.

Unit of measurement=1^σ= .001 second.

P′	MV	n	P″	MV	n	P‴	MV	n
240	38	46	179	28	60	163	20	60

P′, P″ and P‴ denote the tones 100, 250 and 500 respectively. It is at once seen that the reaction-time decreases with the rise in pitch, a result which agrees with that of MARTIUS. The natural inclination is to explain this difference in the reaction-times by the supposition that 10 to 15 vibrations are required before the tone is perceived. If this supposition be true we should obtain the same reaction for all the tones by deducting the perception-time of each from its reaction-time. Suppose we take ten vibrations as representing the inertia of the sense organ ; this would give us the perception-times, 100^σ, 40^σ and 20^σ for the three tones 100, 250 and 500 respectively. Subtracting these perception-times from the total reaction-times given in the table, 240^σ, 179^σ and 163^σ, we get the remainders 140^σ, 139^σ and 143^σ. These remainders fall within the limits of variation and are to be regarded as the same. My results are thus in harmony with the supposition mentioned. MARTIUS obtained results which agree with mine in the fact that the reaction-time decreases with a rise in pitch, but this decrease could in no way be brought into harmony with the supposition that a constant number of vibrations was used up in the latent time.

The conclusions to which my experiments lead are as follows :

First—The reaction-time to tones decreases as the pitch rises.

Second—The view held by EXNER, VON KRIES and AUERBACH and rejected by MARTIUS,—namely, that about 10 vibrations are necessary to the perception of a tone, no matter what its pitch,—is sufficient to explain the differences in the reaction-times for different tones.

EXPERIMENTS WITH ELECTRICAL STIMULI OF DIFFERENT INTENSITIES.

In using electric stimuli the current of the primary circuit after passing through the inner coil was sent through one of the prongs of a fork, kept vibrating electrically, by which it was interrupted at

each vibration. From the fork it was sent through a rheochord composed of seven lengths of wire. Hence it passed to binding posts 2 of the multiple-key mentioned above. The other pole of the battery was connected to binding post 3. When the key is pressed, the circuit is completed. The secondary coil was connected with two electrodes in the reaction-room. One electrode was of zinc covered with cloth, the other of sponge ; both were moistened with a solution of common salt. The other arrangements were the same as in the previous cases. In these experiments the spark-coil was used and the same precautions as observed in former experiments were adopted.

As soon as the multiple-key is pressed the current passes through the primary circuit, being all the time interrupted 100 times per second by the electric fork. This causes a current to pass through the secondary circuit and the person in the isolated room receives 100 shocks per second. As soon as he perceives the shocks he is to react in the usual way. The time is measured between the moment of closing the primary circuit and that of reacting. The intensity of the stimulus can be regulated either by moving the secondary coil nearer to or further from the primary or by weakening the primary current. The former method was not suited to the single experiments but was used to regulate the shock permanently to any desired intensity. Then the different intensities were produced by adjustment of the amount of wire introduced on the resistance-board.

During the experiments six steps of different intensity were taken. The greatest intensity was obtained by shoving the secondary coil sufficiently near to the primary coil until a shock was produced which was not strong enough to startle the reactor and thereby interfere with the reaction-time. When a variation in the intensity of the stimulus was desired, the clamp was transferred to another wire on the resistance-board, causing an increase of resistance of two feet of the wire with each step. When the clamp was on wire No. 6, a resistance of 12 feet of fine German silver wire was thus inserted and a very weak shock was produced.

The experiments were performed in the same manner as the preceding ones. A warning was given previous to each stimulus. The reactions were repeated at intervals of 15 seconds, until a record of 30 reactions was taken, after which a rest of five minutes was given to the reactor. The person experimented on was E. W. Scripture.

The results are seen in table IV.

TABLE IV.

S^{i}	MV	*n*	S^{ii}	MV	*n*	S^{iii}	MV	*n*	S^{iv}	MV	*n*	S^{v}	MV	*n*	S^{vi}	MV	*n*
137	21	19	135	26	15	155	29	17	180	46	26	220	61	24	210	61	22

S^{i} is the shock of greatest intensity; S^{ii}, S^{iii}, S^{iv}, S^{v}, S^{vi} represent shocks of lesser intensity, S^{vi} being the weakest; otherwise the abbreviations are the same as used in previous tables.

It will be seen that there is a slight but constant decrease in the reaction-time with an increase in the intensity of the stimulus. These results coincide with those obtained by other observers.

The conclusion to be drawn seems evident, namely, that in the domain of tactile stimulation by electricity the reaction-time decreases with the increase in the intensity of the stimulus.

EXPERIMENTS ON THE MUSICAL SENSITIVENESS OF SCHOOL CHILDREN

BY

J. A. Gilbert.

Bodily measurements of children have been repeatedly made; their laws of bodily growth have been empirically determined; most important deductions for the equipment and management of schools have been made from them. The senses and intellect of school children have received less attention; most of the work has been confined to investigating the sharpness of vision, the acuteness of hearing (deafness) and the memory powers. The musical sensitiveness has never, I believe, been tested.

By musical sensitiveness is meant the least noticeable difference in the pitch of a tone. Those who can detect a small difference in pitch between two successive tones are more sensitive than those who can detect only larger differences.

Apparatus and methods.

Since the object of the present investigation was not to determine the least perceptible difference in relation to tones of various pitches but was to compare children with one another, a single tone was used throughout the experiments, namely, the tone $\bar{a}=435$ of international pitch. The method was that of minimum gradation. Each experiment was composed of two tones and a judgment as to their likeness. The tone $\bar{a}$ was first sounded, then a tone $\frac{1}{32}$ of a tone higher; the child answered "same" or "different;" $\bar{a}$ was again sounded, then a tone $\frac{2}{32}$ higher; and so on, the second tone being raised $\frac{1}{32}$ each time, until the child had several times declared the tones to be different. Thereupon the second tone was started at the same pitch as the first and in like manner successively lowered. The number of thirty-seconds of difference that were just perceived was noted in the two cases; the average gave the result for a single experiment. Ten experiments were made on each child. The child

was left entirely ignorant of the method of performing the experiment, so as to avoid suggestion of any kind.

The instrument used in making the experiments was composed of an adjustable pitchpipe with an index-arm moving over a large scale.

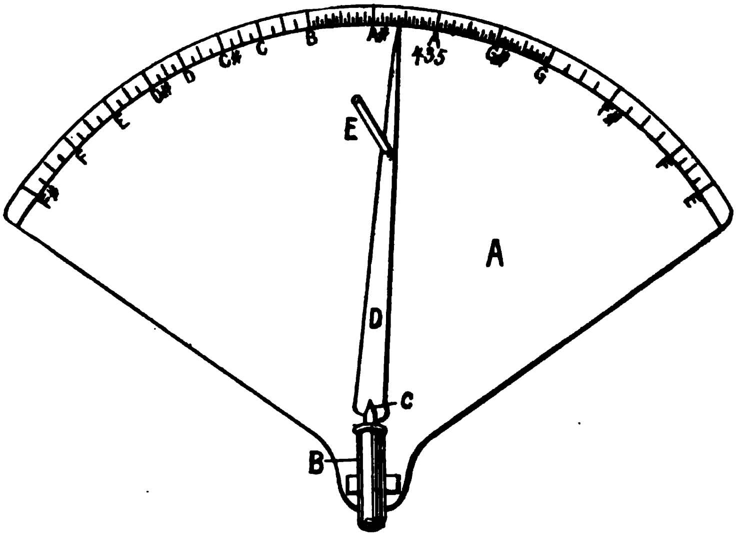

Fig. 28.

The instrument, which may for brevity be called the tone-tester, is shown in figure 28. The fan-shaped plate A is supported by a handle beneath it. The pipe B, fastened to A, contains a vibrating reed whose length is regulated by a tightly sliding clamp, the projecting rod of which is shown at C. This clamp is moved by a lever whose long arm D, with the handle E, extends out over the plate. It is readily seen that, for each different position of the point of the long arm, the vibrating reed will have a different length and the tone produced will be different in pitch. It is also evident that a small difference in pitch corresponds to a large movement of the point of the long arm.

The points on the index for each tone and half-tone were determined by direct comparison with a carefully tuned piano of a piano-dealer. These larger divisions were then divided proportionately into sixteenths. As the large divisions were into half-tones, these divisions corresponded to thirty-seconds of a tone.

The accuracy of the results depended on the accuracy of the instrument and the accuracy of the method. The possible errors of the instrument were as follows: error of tuning, error of graduation, and two errors of change in pitch.

The readings of the instrument must be accurate to 1 unit of the scale ($\frac{1}{32}$ of a tone). The largest allowable error in the instrument is thus $\frac{1}{2}$ a unit of the scale.

As the tuning was performed by observing the freedom from beats, the error from this source could not be above a tenth of a beat per second. The half-tone $\bar{a}$ to $\bar{a}^{\sharp}$ corresponds to nearly 26 vibrations per second; half a space of the scale would indicate about $\frac{3}{8}$ of a vibration. The error is thus less than $\frac{1}{4}$ of a space.

The intermediate graduation was done by eye, and was unquestionably accurate within $\frac{1}{4}$ of a space.

The first error of change in pitch is the error that might be introduced by back-lash in the joint between the levers. To avoid this the lever was always started beyond $\bar{a}$ and moved up to it in the direction in each experiment. With this precaution the error is same practically zero.

The change of pitch due to changing intensity of the blast was mainly eliminated by practice in blowing the pipe. The residual could not be determined, but it was probably negligible in comparison with the others.

All the residuals are much less than the required amount. Their sum was unquestionably within the limit set, and the requirements of precision in the instrument can be said to have been satisfied.

The sources of variation due to mental influences were as follows: influence of the judgment of pitch by changes in intensity, influence of suggestion.

Judgments of pitch are generally made quite without regard to intensity. Measurements of the influence of changes of intensity on the judgments of pitch have never been made; but as great care was taken in blowing the apparatus and as the adult experimenter could regulate the sound with an ear so much finer than that of the children, the variation was probably negligible. Otherwise it enters as one of the factors into the mean variation given in the table.

All chances of suggestion had to be avoided. Children are inclined to follow, almost unconsciously, the slightest indication in making their decisions. Those of the ages nine and upwards were dealt with in groups, each making upon a paper an equality-sign when the two tones seemed the same to him and a cross when they

seemed different. Children of six, seven and eight years of age had to be dealt with individually for fear that they would be influenced by their companions and would not be reliable in their marking. All results were very satisfactory with the exception perhaps of three children aged six whose data were somewhat uncertain on account of lack of attention, independence and decision, or perhaps from the excitement, fear and novelty of the undertaking. These influences of suggestion and distraction are probably the main factors in the mean variations.

Experiments.

Five boys and five girls of each age except 18 and 19 were experimented upon. For the ages 18 and 19 it was possible to obtain only girls. In computing the results the average of all the experiments for a given age was first obtained. The mean variation from this result was noted. Then the children of that age were considered separately, the mean variation from the result for each child being computed. Finally the average of these mean variations was taken. This can be illustrated as follows. Let

$$\begin{array}{llcl} a_1, & a_2, & \ldots\ldots & a_{10} \\ b_1, & b_2, & \ldots\ldots & b_{10} \\ \vdots & \vdots & & \vdots \\ j_1, & j_2, & \ldots\ldots & j_{10} \end{array}$$

be the results for ten children of a given age. The total average will be $(a_1+a_2+\ldots.+a_{10}+b_1+b_2+\ldots.+b_{10}+\ldots.+j_1+j_2\ldots.+j_{10})$ $\div 100$; this is the result given in column D of the table, the first decimal place of the average being retained. The mean variation of the separate measurements obtained in the usual way is given in the column headed MV. This mean variation can be used as an index of the accuracy of the result. The results for each child were obtained by taking the averages $(a_1+a_2+\ldots+a_{10})\div 10=a$, $(b_1+b_2+\ldots+b_{10})\div 10=b$, $\ldots\ldots\ldots$ $(j_1+j_2+\ldots+j_{10})\div 10=j$. The average of these averages is, of course, the same as the total average. The mean variations for $a, b, \ldots.,j$ are then calculated; these mean variations will indicate how much the child's judgments fluctuated owing to the conditions of attention, suggestion, etc. To get at the average effect for the given age the average of these

mean variations was taken; this is given in the table in the column MV′. The last column in the table gives the number of experiments for each age.

TABLE.

Age	D	MV	MV′	n
6	12.3	1.38	1.76	100
7	9.1	.89	3.60	100
8	6.8	.90	1.29	100
9	4.8	1.09	1.14	100
10	6.2	.68	.77	100
11	4.8	1.09	.89	100
12	4.1	.99	.45	100
13	3.7	1.26	.46	100
14	3.5	.97	.94	100
15	5.	1.03	1.11	90
16	4.	.91	.68	50
18	2.6	.74	.93	60
19	2.4	.84	.62	140

D, least perceptible difference in 32nds of a tone.
MV, mean variation for total result.
MV′, average of mean variations for separate children.
n, number of experiments.

The relation of the size of the least perceptible difference to the age is shown in the accompanying curve, fig. 29, in which the figures

on the horizontal axis indicate the ages, those on the vertical axis the least perceptible differences.

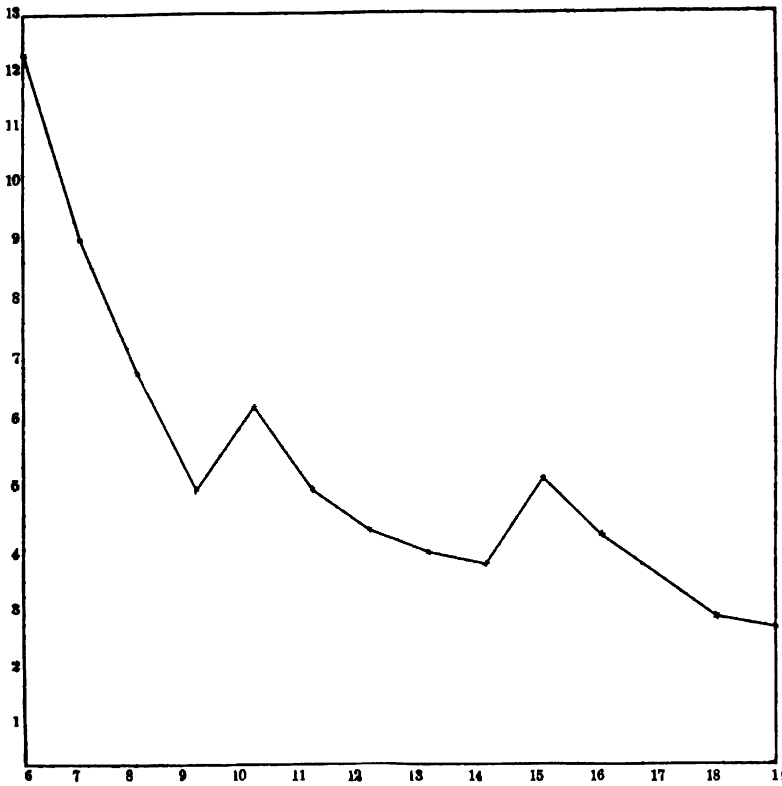

Fig. 29.

CONCLUSIONS.

1. The primary aim in taking up this problem was to discover if there were any who could not distinguish to a half-tone, and if so, to determine the proportion to the total number. The question was of practical importance. Any such children could not, of course, receive the same instruction in music; if the proportion below a certain age were large, musical intelligence could not be expected in the earlier ages.

The results show that the children are fully capable of the task expected of them. The least sensitiveness occurs with children of six years where the average least perceptible difference is 12 thirty-seconds or $\frac{3}{8}$ of a tone. Of all the children examined there were only three individuals whose average exceeded half a tone.

2. It is at once seen that the least perceptible difference decreases with increasing age, i. e. the sensitiveness increases. The sensitiveness increases at first rapidly, but finally becomes almost stationary.

It is a pedagogical principle that the child develops more rapidly during the first ten years of its life than at any other time. Tone discrimination offers no exception to the rule; in the three years from 6 to 9 the child gains in discriminative sensibility more than twice as much as in the whole of the ten years thereafter up to the age of 19. The contrast increases as the number of years taken into consideration is greater, for the child gains more in the three years from 6 to 9 than it can possibly gain during the rest of its life-time.

3. Judging from the way in which mind and body develop under education it would naturally be inferred that the discriminative ability of the child would increase regularly with advance in age. At the ages ten and fifteen, however, occur very abrupt changes. In order to verify the data for those points I repeated the trials on the years nine, ten and fifteen with increased numbers in each age but the second average result varied only 0.3 of a thirty-second from the first average, showing that the curve was true to the facts so far as could be detected.

A similar change in the curve apparently occurred at the age of twenty, but as I only had three subjects of that age I did not feel justified in adding the result to the table, yet this seemingly similar jump at twenty—a leap in the curve from 2.4 up again to 3.2—adds credence to the supposition that there is some periodic change causing it. After twenty years the curve seems to drop again as it did after 10 and 15, but here also the number of persons was insufficient to justify establishing a point on the curve.

It will be noticed that the sudden changes divide the curve into uniform portions from 6 to 9, from 10 to 14 and from 15 to 19.

An explanation for this loss of sensitiveness at certain ages seems difficult.

The change at fifteen is more easily explained, perhaps, than the one at ten, as that is the age at which puberty shows its effects on the system. Although these influences cannot be placed at one certain year, the average lies at 14 years and five months.[1]

Possibly the second teething which occurs at 9 to 12 years of age may have such an influence on mental life as to cause a loss of sensitiveness.

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[1] Eighth Annual Rept. Mass. State Board of Health, 1877, p. 284, Table No. 16.

Similar decreases in ability are to be seen in the results obtained by BRYAN.[1] There is a marked difference between the data of the two periods 9–10 and 14–15 from the data of the other ages. Also in the charts at the close of his article there is an almost invariable change in his curves at the ages 10 and 14, showing that his subjects labored under some set-back or disadvantage at those ages.

[1] BRYAN, *On voluntary motor ability*, Am. Jour. Psych. 1892 V 160, Table VII.

A NEW REACTION-KEY AND THE TIME OF VOLUNTARY MOVEMENT

BY

E. W. SCRIPTURE and JOHN M. MOORE.

In an article on the skin-sense[1] DESSOIR describes an arrangement for use in place of the ordinary telegraph-key in investigating reaction-times. In addition to the gain in convenience he claims that the varying results obtained for sensory and muscular reactions are due to the effect of the construction and manner of use of the ordinary key on the time of executing voluntary movements.

At the request of Prof. Titchener of Cornell University a rough copy of DESSOIR's arrangement was made in our laboratory workshop. It became evident that this arrangement possessed only one good quality, that of portability; with it the arm and hand did not need to be upon a table as with the telegraph-key but could be placed in any comfortable position. With this exception all the disadvantages of the telegraph-key were retained and some further ones added.

The advantage of portability is so great that the problem of the invention of a key that could be used in any position was undertaken. At the same time certain faults of the telegraph-key were to be avoided. There was to be no spring; the key was to act on the make or the break or on both; each contact was to be applicable to either a flexion or an extension movement.

The problem was solved with success. The final form in which the key was made, is shown in fig. 30.

Two hard-rubber slides run on steel guides. The upper slide has a hole to fit the end of the finger. The other has an inclined hole for the thumb, for use when the key is held by the thumb and finger alone. When the key is rested on anything or is held by the other hand, the thumb may be placed against the projecting arm; this arrangement gives a somewhat easier action, as the finger moves more naturally in a plane inclined to that passing through thumb and finger.

[1] DESSOIR, *Ueber den Hautsinn*, Du Bois-Reymond's Arch. f. Physiol. 1892 309.

The binding-post shown at the top carries a platinum contact; that on the upper slide is connected with a contact at each side of the slide; that on the lower slide is connected with a contact pointing upward. The lower slide is fastened at any point by a clamp whose screw is seen to the left in the figure. This determines the range of movement of the upper slide.

The upper slide can make contact at either extreme of its movement. One wire is always carried to the binding-post on the movable slide. To have a break-circuit record with a flexion movement, the other wire is carried to the top binding-post; the thumb and

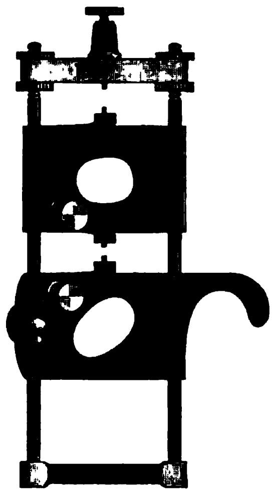

Fig. 30.

finger are held apart at any desired distance; at the least movement of the finger the current is broken. For a break-circuit record with extensor movement the other wire is carried to the post in the fixed slide; the finger is bent till the slides touch. For make-circuit records the lower contact is used with the extended finger, the upper with the bent finger. As the excursion can be made very small by adjustment of the lower slide, the lost time for make-records can be reduced to a minimum.

No spring for holding the contacts is needed. The movable slide is brought into position by the finger and will retain its contact without pressure until disturbed.

While using this key in an exercise on the rapidity of tapping movements of the finger we found that we had the means at hand of making new investigations on the time and extent of voluntary

movement. All previous work in recording taps had been done with an ordinary telegraph-key by which only the moment of the extremity of the downward movement was recorded. By simply connecting the binding-posts of the cross-piece and the fixed slide to one end of the circuit and the post of the movable slide to the other, a record was made of the downward extreme, the upward extreme and the period of rest at each extreme. Moreover, the adjustment of the fixed slide gave any desired extent for the movement.

The method of recording was that described by Bliss on pages 7–10. The key was placed in the primary circuit of the spark-coil and the secondary circuit was sent through the drum and fork. A 500 fork was used, each half-wave indicating 1^σ. A spark-record was made of the kind shown in fig. 6. Records were counted in thousandths, no account being taken of fractions of a half-wave.

The subject of experiment, J. M. Moore, was placed in the isolated room described on pages 2 and 71. The arm rested easily on a table of convenient height. A rod clamped to the table acted as a support for the hand. The rod was grasped by the fourth and fifth fingers, whereby the motion of other muscles than those of the fore finger and thumb was in the main prevented. Three distances were chosen through which the slide was to move, namely, 5^{mm}, 10^{mm} and 20^{mm}. Two series of experiments were made on each distance. The results were combined into the accompanying table.

TABLE.

Distance	E	MV	F	MV	C	MV	n
5^{mm}	33	4	48	8	81	9	70
10^{mm}	40	5	48	9	88	9	60
20^{mm}	53	4	37	5	90	5	40

In this table E indicates the average time of the extensor movement, F that of the flexor movement, C that of the complete movement, MV the average deviation and n the number of records in each case.

With the distance of 5^{mm} the extensor movement required less time than the flexor movement and was much more regular. With 10^{mm}

there was a similar, but smaller difference; the limits of average deviation show that in many cases the slower extensor movements took longer than the faster flexor movement. With 20^{mm} the flexor movement was much more rapid than the extensor movement. It is evident that the point at which they require the same time lies somewhat above 10^{mm}.

These results can perhaps be explained in the following way. When the key was in position the middle finger was placed on the upper cross-piece while the fixed slide was held by the thumb. So whatever the distance, the fore-finger was always brought back to the same bent position. Now, when the distance is very small the finger is never fully extended; thus the extensor muscle has abundant leverage to do a little work in a very short time, while the flexor is not so favored. As the distance increases the flexor slowly gains its power—slowly, because it had never been bent enough to obstruct its movement materially. But while the flexor is slowly increasing in strength, the extensor is losing quite rapidly, for only a few centimeters will fully extend the finger so that its whole power will be lost. When the distance is 10^{mm} the two powers are nearly equal, but with the excess in favor of the extensor. At 20^{mm} the balance of power has changed, and the extensor is so near the end of its strength as to show clearly in the results.

A curious result of the gain in one muscle and the loss in the other is the fact that the total time of vibration, or tapping, varies only about 10 per cent. for distances standing in the relation of 1 to 4.

The time of rest was quite different at the two positions. At the end of the extensor movement the make-spark and the break-spark indicated a period of 2^{σ} to 3^{σ}, occasionally 4^{σ}. At the end of the flexor movement the time of rest was so short that seldom more than one spark was present, indicating a time too short for detection.

In looking over the single records it was found that the time of movement was much more regular than the introspective observation had indicated. At times the observer seemed unable to move the muscle; some unusually long times are found in the record but not so often as the observer supposed.

DRAWING A STRAIGHT LINE: A STUDY IN EXPERIMENTAL DIDACTICS

BY

E. W. SCRIPTURE and C. S. LYMAN.

"The ignorance of the ancients in regard to the art of experimenting, or the low state of development which it reached with them, is one of the causes why their physics lagged so much behind," says Poggendorff in his Lectures on the History of Physics. In comparison with such sciences as mathematics and astronomy physics has achieved most of its progress in modern times. In the very latest times the experimental methods have been carried over from the general science of physics to the technical sciences dependent on it; the science of electrical engineering is built on experiments and measurements, partly taken from the physical laboratories but also to a great extent carried out by practical men for practical purposes.

In psychology the first real progress since Aristotle began when Fechner showed the possibility of experimental methods. "With the introduction of experiment, the trustworthy application of the method of introspection became for the first time possible" (WUNDT, Physiol. Psy., 4 ed., I 4). Psychology to-day is a science of experiment and measurement. The time seems at hand when applied psychology should also become an exact and trustworthy science. Pedagogy, or the science of education, is in great extent based on psychology. It will not, however, do to wait for the crumbs that fall from the psychologist's table; he is thinking of other matters than practical applications. Pedagogy, moreover, has its own peculiar problems which must be solved in special ways. Can pedagogy make use of experiments in solving any of its problems?

We have chosen such a simple matter as the drawing of a straight line and have tried to gain information on the subject by making experiments. The main object was to see if we could really experiment on a pedagogical subject; at the same time we hoped to make a contribution to our knowledge of methods of drawing. No wide generalizations have been attempted; we have gathered a few facts and drawn the proximate conclusions from them. We firmly

believe, however, that not only can other questions that arise in regard to drawing be settled by experiments conducted in the proper way, but also that the same method can be extended with little difficulty to innumerable questions that arise in education.

Ten boys of the upper grammar grade of the school of Manchester, Conn., were chosen so as to be of as nearly the same age as possible, the average age being 13 yrs. 3 mos. They had had some instruction in drawing throughout the various grades, but not to such an extent as in the city schools; some of them from the country districts had had very little.

The boys all sat at their desks in just the same positions. A sheet of paper 7 in. long by 4 in. wide was placed before each. In the middle of the sheet were two dots 100^{mm} apart lengthwise of the paper. At a given signal each boy drew a straight line between the dots. Afterwards a ruler was laid on each sheet so that its edge cut the dots. With a pair of dividers the greatest deviation of the line drawn from the true straight line was found. The dividers were then applied to a scale and the results recorded in millimeters, the tenths of a millimeter being estimated. The additional figure was retained in the averages, the last significant figure, however, is tenths of a millimeter.

The experiments were performed under certain sets of conditions. In the first sets the boys sat squarely in front of the desk, holding the pencils in the usual way grasped near the middle. The line was drawn with a single movement of the pencil, without going over it a second time or erasing. The first line drawn was horizontal, i. e. parallel to the front surface of the body. On the second set of papers the line drawn was vertical, the other conditions remaining the same. In the third set the line was 45° to the right, in the fourth 45° to the left. The positions of these lines can be thus shown: 0° $\rightarrow$ 270° $\downarrow$ 45° $\nearrow$ 325° $\searrow$. The arrows indicate the direction of movement of the pencil. In calculating the deviations, or errors, those deviations toward an excess of the angle were called +, those toward the primary position of the line −; in the table the corresponding terms, over, under, to the right and to the left are added. The total error Σ is the difference between the maximum + deviation and the maximum − deviation, i. e. the amount of the + deviation added to the amount of the − deviation. It is not the distance between the extremes of the deviations of the same line.

In further sets of experiments the position of the boys was changed, the right side being placed toward the desk. Still other

TABLE.

Varied condition	0° + over	0° − und'r	0° Σ	45° + left	45° − right	45° Σ	270° + right	270° − left	270° Σ	325° + right	325° − left	325° Σ
Facing, mid grip, steady	0.86	1.10	1.96	0.75	1.61	2.36	1.24	0.51	1.75	1.16	2.36	3.52
Right side, mid grip, steady	1.10	0.51	1.61	2.22	0.43	2.65	1.41	0.94	2.35	1.06	1.64	2.70
Facing, near grip, steady	0.56	1.43	1.99	2.06	0.76	2.82	0.83	1.20	2.03	0.76	1.75	2.51
Facing, far grip, steady	0.53	1.12	1.65	1.66	0.31	1.97	1.14	0.63	1.77	1.30	1.67	2.97
Facing, mid grip, progressive	1.24	0.94	2.18	0.96	1.45	2.41						
Facing, near grip, progressive	1.33	0.79	2.12	1.16	1.03	2.19						
Facing, far grip, progressive	1.40	1.15	2.55	1.16	1.07	2.23						
Average	1.00	1.01	2.01	1.42	0.95	2.37	1.16	0.82	1.98	1.07	1.86	2.93

sets were the same as the first excepting the grip, the pencil being held near the point. In the next sets the pencil was held far from the point, otherwise the conditions were the same as in the first set. In the next three sets the lines were drawn by progressive movements instead of a steady movement. Two inclinations were chosen, 0° and 45°. The first set was done with the middle grip of the pencil, the second with the near grip, the third with the far grip.

Some experiments were tried with other positions than those of facing and right side, but the positions were so awkward that no results worth tabulating were obtained.

From the results given in the table we can draw a number of conclusions. The facing position is more favorable for horizontal (1.96) and vertical lines (1.75) than it is for inclined lines (2.36, 3.52). The right-side position is also more favorable for horizontal (1.61) and vertical (2.35) than for 45° (2.65) and 325° (2.70). This is what we might expect as a result of Listing's law according to which the

eye moves more easily upward, downward, right and left (i. e. vertically and horizontally), than in intermediate positions.

In drawing horizontal lines and 325° lines the right-side position is more favorable than the facing position; for the others facing is preferable. This is perhaps to be explained by the fact that the fore arm swings around the elbow in a curve which in order to produce a straight line must be compensated by a backward and forward movement of the upper arm around the shoulder. In the facing position with the paper directly in front the fore arm touches the body at the start and the hand is bent at the wrist. As the arm moves, it becomes freer and a more natural position is assumed. This change in the manner of carrying the arm would tend to introduce uncertainty into its movements. With the arm raised upon the desk in the right-side position it is brought clear of the body, and the line can be executed in one sweep. In drawing the 45° line the arm is just as free in the facing as in the right-side position and we find little difference in the results. In drawing the vertical line we would naturally expect much greater accuracy when the motion is a simple forward or backward movement of the arm around the shoulder, as in the facing position, than when the arm has to undergo complicated adjustment with the elbow raised. Why there should be a difference with the 325° line it seems impossible to say. Both positions, facing and right side, are on the whole equally favorable for accuracy, as can be seen by taking the average of the total errors, Σ, 2.40 for facing, 2.33 for right-side.

Holding the pencil far from the point is in general the most accurate method (average of Σ 2.09); near the point is as accurate as the middle grip (2.40). With the pencil far from the point the line is drawn with a smaller movement of the hand, which would give a better result than a larger movement requiring adjustments from elbow and shoulder. For horizontal lines the far grip is the most accurate (1.65 against 1.96, 1.99); for 45° the same is true (1.97 against 2.36, 2.82); for vertical lines the middle and the far grips are the same (1.75, 1.77), the near grip is unfavorable (2.03); for the 325° line the near grip is the best (2.51), the far grip is next (2.97), the middle grip is very unfavorable (3.52). That the 325° line forms an exception to the advantages of the far grip and is much less regular than the others, is evidently connected with the awkward contraction of the fingers in this direction.

In progressive lines experiments were made only on 0° and 45°. With the middle grip the result is less accurate for both horizontal

(2.18 against 1.96) and inclined lines (2.41 against 2.36) than for steady lines. With the near grip it is less accurate for horizontal (2.12 against 1.99) but more accurate for inclined lines (2.19 against 2.82). With the far grip it is much less accurate for both (2.55 against 1.65; 2.23 against 1.97). In general we would expect a progressive line produced by complicated movements to be less accurate than a steady line. This is the case except in the inclined line with near grip; for this exception we are unable to find any reason.

When we compare the two kinds of errors + and − we find that for 0° they are in general equal (1.00 to 1.01). In the facing position with the steady line the − error is larger, the + errors being 0.86, 0.56, 0.53, the − errors being 1.10, 1.43, 1.12. With the progressive line the reverse is the case: + errors 1.24, 1.33, 1.40; − errors 0.94, 0.79, 1.15. In general the facing position gives a tendency to − errors, the right-side position to + errors.

With the 45° lines the general tendency is to + errors (1.42 against 0.95). This is to be expected as the 45° line is an arc drawn by the forearm with a correction introduced by the upper arm; when we try to draw two arcs of a circle instead of the straight line we generally make the  arc too curved and the  too flat. With progressive lines the tendency is for some reason just the reverse but is in general much less. In the right-side position there is an overwhelming tendency to + errors (2.22 against 0.43).

With the 270° lines the general tendency is to + errors. The middle grip and the far grip give + errors, the near grip − errors.

With the 325° lines the − errors predominate in almost every case.

It is interesting to note which inclinations give on the whole the most accurate lines. By comparing the values for Σ we find that the most accurately drawn line is the 270° or downward vertical line and the least accurately drawn the 325° or left inclined line.

The reasons for many of these facts are still greatly matters of conjecture to be settled only by careful investigation of the action of the separate muscles of the arm and eye in each case.

SOME NEW PSYCHOLOGICAL APPARATUS

BY

E. W. Scripture.

In starting the Yale laboratory it was deemed best to provide ample facilities for the repair and construction of apparatus by establishing a serviceable workshop-equipment. In addition to the large amount of work done for the investigators of special problems, several pieces of apparatus of general use were invented and manufactured. Some of these, the multiple key (first model), the reaction-key and the switch-board have been described in the preceding pages. There are, however, three others that are deserving of mention.

In the first model of the multiple key, the two levers were hung on different axles; consequently the arcs described by the contact-points were not concentric. For most purposes this made little or no difference. For two of the numerous uses of the key it appeared quite desirable to avoid this difference of centers. In the first place it is geometrically evident that in using the contact E-U (fig. 11) the upper point will slide sideways slightly as the key is depressed. This makes slight variations in the resistance to the current passing through the points. These variations are not noticeable except in a telephone-circuit; there the result is to produce a grating noise in addition to the tone or noise sent through the telephone. The other objection is a somewhat similar one in regard to the contact D-S-T. The point T slides along the spring S and causes variations in the strength of the current.

To remove these objections a new key was built in which the lower lever works around the same axis as the upper one, so that any point of the upper lever striking the lower one will always touch it at the same place, no matter how far it is moved.

The new key is shown in fig. 31. The lower lever has two projecting arms that are hung on the axle of the upper lever. There is no necessity for the spring S (fig. 11) in the first key; consequently both contacts 7 and 3 are alike. Experience having shown the mercury cup to be better than the contact N-R of the first key, the latter has been omitted. Figures opposite the various contacts indicate the binding-posts to which they are connected.

The vital point in the construction of the key is the concentricity of the levers. In the one already made for Clark University this has been carried out so well that both sets of contacts 7–8 and 3–4

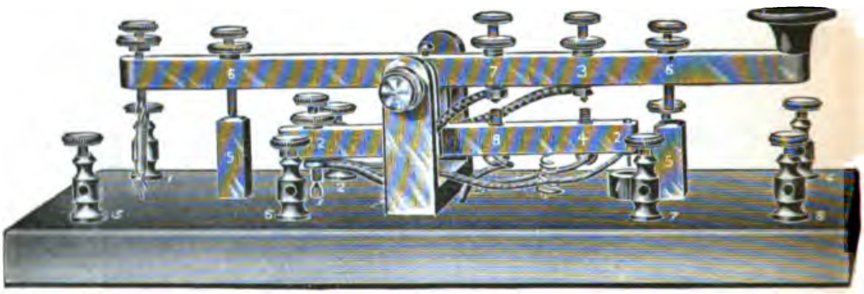

Fig. 31.

can be adjusted to strike at the same time and maintain their contact through an arc of 10°. The rubbing of the upper contact against the lower one, as tested by the telephone, has been totally eliminated for 7–8 throughout the arc and for 3–4 throughout a somewhat smaller one.

Another piece of apparatus constructed is a pendulum-contact.

Those who have used mercury contacts for clock-pendulums well know the continual trouble that they give when placed in the center of the arc of swing at the extremity of the pendulum. Relief has been sought by resorting to platinum contacts made when the pendulum is at the extremes of its arc. The sources of error in this method have made it almost inapplicable in scientific work.

After wasting considerable time and money in these two ways, a contact-apparatus was invented which made platinum contact in the middle of the arc of swing. In this way the advantages of solid contact and mid-arc contact were gained together.

This clock-contact is shown in fig. 32. The support A carries the horizontal metal piece B with the binding-post C. The rubber block D, fastened to B, carries the metal arm E, which by means of its axle is in contact with the binding-post F. At the end of E there is a platinum point G which rests on another platinum point at the end of the screw H. This instrument is placed on the floor of the clock just far enough in front to clear the pendulum. A fine wire I is run from the arm E to a pin on the pendulum-arm J. If H is adjusted so that the platinum points just touch when the pendulum is at rest, any movement will break the circuit. When the pendulum is swinging, contact is made only at the lowest point of the arc.

Owing to the light weight of the parts and the smallness of the movement, the friction is exceedingly small.

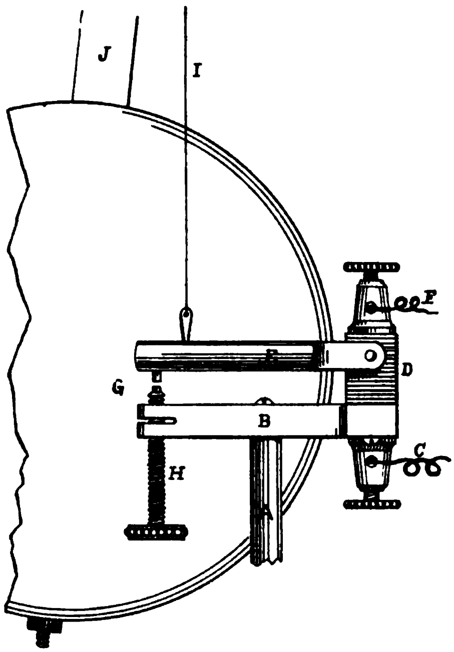

Fig. 32.

The third instrument to be described is a new chronograph. The electric drum mentioned by Bliss on p. 9 was a temporary arrangement which did such excellent service that the construction of a thoroughly durable apparatus was decided upon.

The drums used for physiological work, such as the Baltzar kymograph, are made to run at slow rates of speed. Those used for physical purposes, such as the König drum, are not made for continuous running. For psychological work there is need of a very rapid drum running continuously as long as desired. In the Wundt[1] chronograph a high rate of speed is obtained by using clockwork as the motor power. Such an arrangement is very expensive and is unsatisfactory in several ways.

The drum shown in fig. 33 is either a hand-drum or an electric drum. The cast-iron base is supported on three fixed and one adjustable leg. The drum itself runs on hardened steel centers

[1] Phys. Psych. 3 ed. II 279.

held by two uprights bolted to the base. When the drum is to be turned by hand, a large wheel is placed on the axle as shown in the figure. With this weight the drum will run several minutes with

Fig. 33.

one impulse of the hand. When an electric motor is to be used, the wheel is removed and a pulley is placed on the axle. The motor is fastened to the base by a slide and bolt.

The carriage for the time-marker is mounted on rails planed like those of a lathe-rest. Rigidity is attained by the conical support for the rod.

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ON MEAN VALUES FOR DIRECT MEASUREMENTS,

BY

E. W. SCRIPTURE.

SOURCES OF ERROR.

In making measurements we are subject to errors which can be classed as: 1. errors of scale, 2. errors of observation, 3. errors of definition, 4. errors of number, 5. errors of calculation.

1. *Errors of scale.* Since the divisions of the standard used in measuring come into consideration, it must be determined on every occasion what aliquot portion of the unit, or what ultimate sub-unit shall or can be finally considered. Let this sub-unit be r. On the axis of X (fig. 1) let the value of the quantity measured be

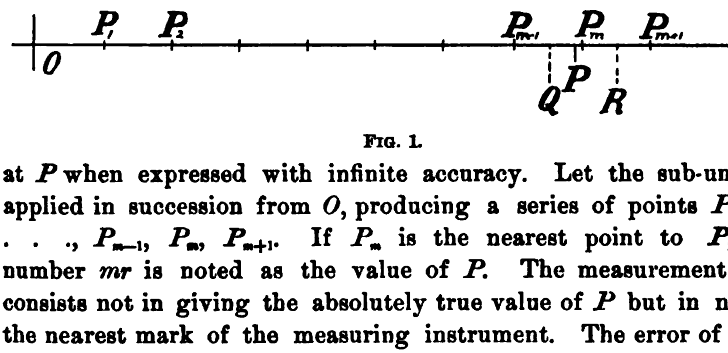

FIG. 1.

at P when expressed with infinite accuracy. Let the sub-unit be applied in succession from O, producing a series of points P_1, P_2, . . ., P_{m-1}, P_m, P_{m+1}. If P_m is the nearest point to P, the number mr is noted as the value of P. The measurement thus consists not in giving the absolutely true value of P but in noting the nearest mark of the measuring instrument. The error of scale $P-P_m$ in any particular case cannot be known, as all the points between Q and R will be denoted by the same result. It is usually assumed that results between $Q = P_m + \frac{r}{2}$ and $R = P_m - \frac{r}{2}$ are equally probable and that in the long run the result $P_m = mr$ will differ but little from P.

The assumption of equal probability for an error of $+\frac{r}{2}$ and $-\frac{r}{2}$ is not strictly correct. Particularly wrong is the further assumption that all values between $+\frac{r}{2}$ and $-\frac{r}{2}$ are equally probable.

If all other sources of error be made negligible, i. e. if the errors of observation and of definition be made sufficiently small, the re-

sulting value P can be determined in each case with an approximation so close that it can be regarded, comparatively, as the true value. The scale-readings for a set of results can then be compared with the "true" values. Thus a series of differences

$$P^{(1)}-P_m=V_1;\ P^{(2)}-P_m=V_2;\ .\ \ .\ \ .;\ P^{(n)}-P_m=V_n$$

are obtained, which can be called the errors of scale.

The real values of the measurements are thus distributed over a region $P^{(1)}-P^{(n)}$ for which one value P_m is read as a representative. It thus becomes a question how well P_m represents the true values.

The influences that affect the error of scale, whereby the actual reading may be too great or too small, may be said to be of two kinds: 1. instrumental, 2. psychological.

An example of the former may be seen in all instruments in which the reading depends on a bar dropped into the teeth of a rack, as in the Hipp chronoscope. If the edges of the bar and of the teeth were infinitely sharp, the bar would (other sources of error supposed absent) drop and slide down just as often on one side of a tooth as the other. If, however, the edges are rounded, as they must eventually become by wear, the inertia of the body in motion, whether the bar or the rack, will carry the bar constantly to one side whenever it strikes on the rounded edge. Thus in the Hipp chronoscope the readings are all slightly too large. A reading of m^σ is supposed to represent all values between $(m+\frac{1}{2})^\sigma$ and $(m-\frac{1}{2})^\sigma$ whereas in an old instrument m^σ may be the reading possibly for $(m+\frac{1}{4})^\sigma$ to $(m-\frac{3}{4})^\sigma$.

An example of the latter is found in the familiar case of reading in tenths of the index-unit on a graduated scale, as in getting thousandths of a second from a tuning-fork curve in hundredths. A large portion of the work on the least perceptible difference might be used to determine the law of frequency for the error of scale.

The assumption of equal probability for an error of $+\frac{r}{2}$ and $-\frac{r}{2}$ is not strictly correct, as the law of probability followed by the measurements in general will be followed here also. It has been proven, however, that the error introduced by the assumption is of the second order of smallness as compared with the error of scale.[1]

2. *Errors of observation.* In making measurements the error of

[1] Lehmann-Filhés, *Ueber Ausgleichung abgerundeter Beobachtungen*, Astr. Nachr., 1889 CXX 305.

Pizzetti, *Sur la théorie des observations arrondies*, Astr. Nachr, 1890 CXXIV 33.

scale can be made so small as to be negligible in comparison with all other sources of error. The measurements $x_1, x_2, \ldots, x_n$ can be considered as having been recorded with practically perfect accuracy. They will disagree owing to sources of error thus classified by LAMBERT:[1] inaccuracy of the instruments, lack of care of the observer, dullness of the senses, and circumstances of the observation. The errors of observation resulting herefrom falsify the separate measurements. As these sources of error are made smaller the values of x differ less from one another and we can suppose that they would ultimately tend toward a value X which is usually called the true value of x. If we could know this true value, the separate errors of observation would be given by

$$V_1=x_1-X;\ V_2=x_2-X;\ \ldots;\ V_n=x_n-X.$$

As we cannot know X we cannot know the values of V. If we take some representative value R, we obtain

$$v_1=x_1-R;\ v_2=x_2-R;\ \ldots;\ v_n=x_n-R.$$

The best we can do is to determine that the value R shall not differ from X by more than a given amount. This is the problem of adjustment of measurements as it is usually proposed in works on physical measurements.

3. *Errors of definition.* When the errors of scale and of observation are made so small as to be entirely negligible, the results $x_1, x_2, \ldots, x_n$ will be practically true as far as these errors are concerned. They will still not agree owing to the infinite number of factors entering into the definition of the quantity. No matter how carefully we define it, we can never make it absolutely complete.

Let the quantity measured be the height of the American soldier. The object is already limited to a nation, a sex, a class and a range of ages. Let the unit of scale be 0.001^m and the method of measurement be a blunt point descending on an arm supported without shake by a rigid vertical bar. The recorded heights will extend over a range of 0.60^m. The height of the American soldier is thus uncertain to that amount.

Let the quantity be further limited to a given age, say 30 yrs., and a given nativity, say Massachusetts; this may render the result definite to 0.30^m. Let it be further confined to a given company of a given Massachusetts regiment at a given day. The result might well be uncertain to 0.15^m.

[1] LAMBERT, *Theorie d. Zuverlässigkeit d. Beobachtungen u. Versuche*, Beyträge z. Gebr. d. Math., I 424.

These limitations are still indefinite. As the age may range over 12 months and the constituency of the company might change, let the measurement be limited to a given individual, and let 10 measurements be made in the manner prescribed. The results will depend on the altitude and inclination of the head. Even if these be defined as the extreme height while standing on the heels, the individual will, owing to practice and to fatigue, never stand twice alike. Let the required height be the maximum attainable under any conditions; the results will vary unless the occasion be fixed at a given time. Although the subject has never been investigated, the varying atmospheric conditions during the day undoubtedly affect the activity of the nervous system and thus the tension of the muscles maintaining the upright position. Even when limited to a definite occasion the results will vary unless the pressure of the point on the skin be defined. Here, however, the limits of accuracy of the proposed method of observation will be reached; there will probably be no changes comparable to the inaccuracy of the observer's eye in adjusting the point. A finer method with multiplying levers and air-transmission by Marey capsules will reveal continual, though minute, fluctuations. The error of definition could in this manner be reduced probably to 0.0001^{m} by taking the height thus indicated at a given instant. With still finer methods familiar to physicists the instant of time would necessarily be more closely defined. Enough, however, has been said to show that owing to the impossibility of defining with infinite accuracy the quantity measured, the true value of the quantity is to be considered as that which is obtained with an accuracy sufficient for the purpose in hand; whether it be a general figure by which to compare the American with the French soldier, by which to compare those from the different states, by which to designate a particular individual, or by which to determine the individual's change at each instant.

Let it be required to determine the reaction-time of a given individual on a given occasion to a given stimulus of a given intensity. Let the external conditions be further defined by perfect quietness and perfect darkness; let the air-supply be of a given quality, the pulse-rate of a given frequency, etc. Let the method of recording be accurate to 1^{σ}, i. e. the error of observation shall be less than that amount. Even under these circumstances the results will vary owing to the continually changing subjective conditions of attention, fatigue, etc., which are still beyond control. It is only in exceptional cases that the disagreement can be reduced below an average

of 10^{σ}. As the methods of psychology are perfected we shall be able to define and control each of the subjective conditions influencing the time, until we can define the reaction-time so that it shall not vary to the extent of 1^{σ} under the given conditions.[1] Of course, when this occurs the errors of recording must be proportionately small.

When the error of definition is negligible in comparison with the errors of observation in direct measurements, physicists speak of the "true" value of the quantity which would be attainable with an infinitely accurate method of observation. As physicists are able in many fundamental cases, e. g. length, time, etc., to define more accurately than they can observe, the "true" value in this sense is often sharply distinguished from an average value. There is a "true" value for the height of the barometer for any given minute because we cannot observe finely enough to detect the continuous fluctuation.

Enough has been said, I think, to indicate that the distinction sometimes made between the average of observations and the mean of a series of quantities[2] is really a distinction between the mean of a series of quantities subject to variation in observation and the mean of a series of quantities subject to variation in definition.[3]

4. *Errors of number.* The result of a measurement is recorded with a definite number of decimal places, whereas a perfectly true value would require an infinite number. The possible numerical error of any result recorded to a places is 0.5β, where β is 1 unit in the a place. Owing to the partial cancellation of these errors in combining measurements, the mean numerical error in the a place is 0.25β.

5. *Errors of calculation.* The errors caused by mistakes in reading scale-numbers, in writing them down and in performing the necessary arithmetical calculations can be treated by the principles of probability.[4] For the sake of simplicity they will be left out of consideration here.

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[1] SCRIPTURE, *Accurate work in psychology*, Am. Jour. Psych, 1894 VI 427.

[2] QUETELET, Théorie des prob., Lettre XI, Bruxelles 1846.

HERSCHEL, QUETELET *on probabilites*, Edinburgh Rev., 1850 CLXXXV 1; Essays, 365 (404), Lond 1857; also in QUETELET, Physique sociale de l'homme, I 35, Bruxelles 1869.

JEVONS, Principles of Science, 2. ed, 362, Lond. 1887.

[3] VENN, Logic of Chance. 2. ed., 96, Lond. 1876.

[4] HOFMANN, *Ermittelung der Tragweite der Neunerprobe bei Kenntniss der subj. Genauigkeit des Rechnenden*, Zt. f. Math. u. Phys., 1889 XXXIV 116.

EMMERICH, *Zur Neunerprobe*, Zt. f. Math. u. Phys., 1889 XXXIV 320.

Representative values.

Owing to the various sources of error the measurements x_1, x_2, . . ., x_n will generally disagree. The disagreeing results of the measurements, regarded in themselves, are actual concrete matters of fact. For practical reasons these values are replaced by one value deduced from them on some given principle. The proper selection of the representative value requires a clear conception of the purpose for which it is to be used and of the method of deduction.

It is sometimes desirable to choose an extreme value to represent the actual ones, e. g. the highest mast to be allowed for in building a bridge, the smallest difference that might be noticed, the largest variation for a given degree of probability; but for the purpose here under consideration some mean value is desired and to this the discussion will be confined.

Proposed means.

Means have been proposed on four principles: 1. on consideration of the properties of a geometrical or material figure employed in representing the results and their probabilities; 2. on the principles of probability *a priori;* 3. on the possible ways of practically calculating means; 4. on certain relations of the powers of the variations.

1. *General analytical or geometrical deduction.* If we denote the number of occurrences of the result x_k by m_k, where $k = 1, 2, \ldots, n$, then the quotients

$$\frac{m_1}{m}, \frac{m_2}{m}, \ldots, \frac{m_n}{m},$$

where $m = m_1 + m_2 + \ldots + m_n$, will denote the relative frequencies of $x_1, x_2, \ldots, x_n$ respectively. These frequencies stand in the relation

$$m_1 : m_2 : \ldots : m_n.$$

When the individuals are taken at random, as m is made greater, the quotients tend toward definite limits,

$$\phi(x_1), \phi(x_2), \ldots, \phi(x_n),$$

known as the probabilities of $x_1, x_2, \ldots, x_n$ respectively.

a. Selection on the basis of $y = \phi(x)$. Let the given values be laid off on the axis of X (fig. 2) and the ordinates $y_1, y_2, \ldots, y_n$,

be erected proportionately to $\phi(x_1)$, $\phi(x_2)$, . . ., $\phi(x_n)$. For continuous values,

$$y = \phi(x) \tag{1}$$

will be the expression for the curve or law of frequency or probability.

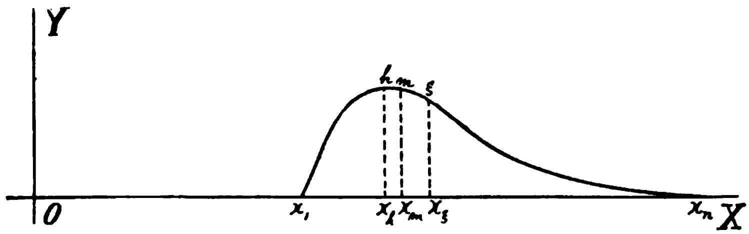

FIG. 2.

The probability of any value x may be regarded as the probability of a value falling between x and $x + dx$. This will be represented by the area inclosed by the curve over the base dx with the mean ordinate $\phi(x)$. The total area is

$$W = \int_{x_1}^{x_n} \phi(x)dx. \tag{2}$$

Since the probability of a result outside of the given limits is not 0 but is infinitely small, it is justifiable to write

$$W = \int_{-\infty}^{+\infty} \phi(x)dx. \tag{3}$$

The form (2) will, however, be retained here as the extension of the limits has occasioned some misunderstanding.[1]

Since it is certain that all values are included between $-\infty$ and $+\infty$,

$$W = 1. \tag{4}$$

The value of maximum probability is the abscissa x_h of the highest point of the curve. It is found by putting

$$\frac{d\phi(x)}{dx} = 0$$

and taking that one of the resulting values for which $d^2\phi(x)/dx^2$ is negative.

[1] CATTELL, *On errors of observation*, Am. Jour. Psych., 1893 V 287.

The value of mean area x_m is the abscissa whose ordinate divides the area of the curve into two equal parts, and is found from

$$\int_{x_1}^{x_m}\phi(x)dx = \int_{x_m}^{x_n}\phi(x)dx = \tfrac{1}{2}W = \tfrac{1}{2}. \tag{5}$$

This value of mean area was called by LAPLACE[1] the value of middle probability. For the sake of brevity the name can be shortened into "middle value."

The probabilities $\phi(x_1)$, $\phi(x_2)$, . . ., $\phi(x_n)$ can be regarded as parallel forces acting on the points x_1, x_2, . . ., x_n of a straight line. The centroid or mean center will be at

$$x_\xi = \frac{\Sigma\phi(x)\,x}{\Sigma\phi(x)} \tag{6}$$

If $\phi(x_1)$, $\phi(x_2)$, . . ., $\phi(x_n)$ be regarded as masses at the points of a straight line, the position of the center of gravity will be expressed by the same equation.

For continuous values,

$$x_\xi = \frac{\int_{x_1}^{x_n} x\phi(x)dx}{\int_{x_1}^{x_n}\phi(x)dx}; \tag{7}$$

or, on account of (2) and (4),

$$x_\xi = \int_{x_1}^{x_n} x\phi(x)dx. \tag{8}$$

This equation also represents the abscissa of the center of gravity of an area of uniform density bounded by the axis of X and the curve $y = \phi(x)$.

These values are represented in fig. 2. The highest point of the curve is at h; its ordinate is x_h. The ordinate m, x_m divides the area of the curve into halves. The mean center or center of gravity is on ξ; its abscissa is x_ξ.

b. Selection on the basis of $Y = \int\phi(x)dx$. Starting with x_1 on the axis of X, erect the ordinate $Y_1 = \phi(x_1)$. At $x = x_2$, erect $Y_2 = \phi(x_1) +$

[1] LAPLACE, *Mémoire sur la probabilité des causes par les évènements*, Mém. de math. et de phys. par divers savans, Acad. Paris, 1774 VI 621 (636).

$\phi(x_3)$; at x_k, ($k=1, 2, \ldots, n$) erect $Y_k=\phi(x_1)+\phi(x_2)+\ldots+\phi(x_k)$. The unit of abscissa is here, as before, dx, and just as in the previous case, these values can be transformed into continuous ones. Thus

$$Y_k=\int_{x_1}^{x_k}\phi(x)dx. \tag{9}$$

This is the integral curve for $y=\phi(x)$, and may be plotted or drawn directly from it.[1]

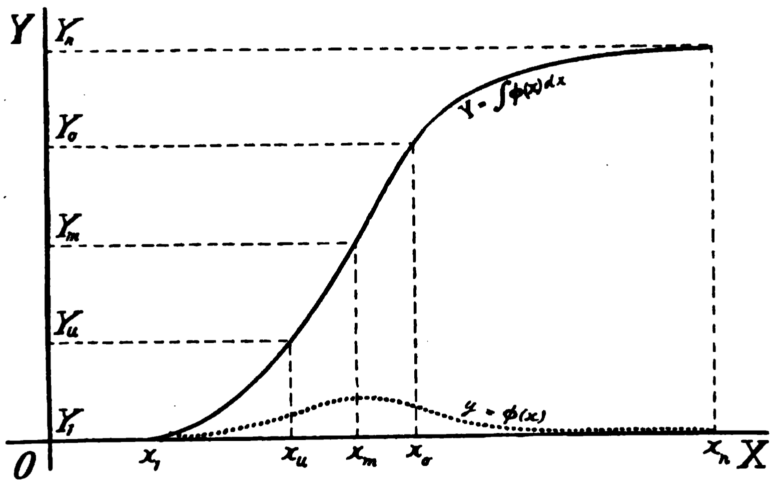

FIG. 3.

The difference between the ordinates at the beginning and end of the curve will correspond to the total area of the frequency curve. Thus

$$Y_n-Y_1=\int_{x_1}^{x_n}\phi(x)dx=W=1.$$

The value x_m whose ordinate Y_m is halfway between Y_1 and Y_n is determined by

$$Y_n-Y_m=Y_m-Y_1$$

or

$$\int_{x_1}^{x_m}\phi(x)dx=\int_{x_m}^{x_n}\phi(x)dx=\tfrac{1}{2}W=\tfrac{1}{2}. \tag{10}$$

This value x_m is evidently the value of middle probability noted above.

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[1] ABDANK-ABAKANOWICZ, *Les integraphes, la courbe intégrale et ses applications*, Paris 1886.

The over-quartile x_o and the under-quartile x_u are determined by

$$x_o = f\left(\frac{3(Y_n - Y_1)}{4}\right), \qquad x_u = f\left(\frac{Y_n - Y_1}{4}\right)$$

The octiles are determined by

$$x_k = f\left(\frac{k(Y_n - Y_1)}{8}\right), \quad (k = 1, 2, \ldots, 8),$$

These may be used as characteristic values to indicate the form of the curve.[1]

The percentiles

$$x_c = f\left(\frac{c(Y_n - Y_1)}{100}\right), \quad (c = 1, 2, \ldots, 100)$$

have formed the basis of the method of percentile grades.[2] In practice the percentiles generally become vigintiles or deciles.

Galton, who first introduced the practical use of this method of considering measurements, treats them in a way which, mathematically stated, is as follows. With any arbitrary unit on the axis of X, erect in succession the ordinates

$$Y_1 = x_1; \; Y_2 = x_1; \; \ldots; \; Y_{m'} = x_1,$$

extending the processes till the value x_1 has been used as many times as it occurs in the set of results. Likewise let

$$Y_{m'+1} = x_2; \; Y_{m'+2} = x_2; \; \ldots, \; Y_{m'+m''} = x_2,$$

where x_2 has occurred m'' times. Repeating this process we have

$$Y_{m'+m''+1} = x_3; \; Y_{m'+m''+2} = x_3; \; \ldots; \; Y_{m'+m''m'''} = x_3;$$

. . . .

$$Y_{m'+m''+ \ldots +m_{r-1}+1} = x_r; \; Y_{m'+m''+ \ldots +m_{r-1}+2} = x_r; \; \ldots;$$
$$Y_{m'+m''+ \ldots +m_{r-1}+m_r} = x_r;$$

. . . .

[1] Galton, Hereditary Genius, 33, London 1870.

Galton, *Statistics by intercomparison*, Phil. Mag., 1875 (4) XLIX 33.

Galton, Inquiries into Human Faculty, 51, New York 1883.

McAlister, *The law of the geometric mean*, Proc. Royal Soc. London, 1879 XXIX 367 (374).

Edgeworth, *Problems in probabilities*, Phil. Mag., 1886 (5) XXII 371 (374).

[2] Galton, Natural Inheritance, London 1889. (I have not seen this work.)

Bowditch, *Physique of women in Massachusetts*, XXI. Ann. Rept. Mass. State Board of Health, 285, Boston 1890.

Bowditch, *Growth of children*, XXII. Ann. Rept. Mass. State Board of Health, 479, Boston 1891.

Seaver, Manual of Anthropometry, 1. chart, New Haven 1890.

Geissler, *Ueber die Vorteile der Berechnung nach perzentilen Graden*, Allg. stat. Arch., 1891–1892 452.

$$Y_{m'+m''+\cdots+m_{n-1}+m}=x_n;\quad Y_{m'+m''+\cdots+m+_{n-1}+3}=x_n;\ \ldots\ ;$$
$$Y_{m'+m''+\cdots+m_{n-1}+m_n}=x_n.$$

Thus in general Y_{m_k} is the ordinate for

$$X_{m_k}=\sum_{m'}^{m_k} m,\quad (k=1,2,\ldots,n).$$

By considering the whole interval covered by X to be $m=m'+m''+\ldots+m_n$, with the sub-intervals $\frac{m'}{m}, \frac{m''}{m}, \ldots, \frac{m_n}{m}$ we have for any point X_k

$$X_k=\sum_{m'}^{m_k}\frac{m_i}{m},\quad (i=1,2,\ldots,k).$$

For continuous values this evidently becomes of the same form as (9) and would become exactly the same if the Y-axis had been used in place of the X-axis. GALTON's elementary illustration[1] of a group of men placed side by side in order of size with a curve just touching the tops of their heads has led to the use of the axis of Y for the values of $x_1, x_2, \ldots, x_n$. As GALTON's method and illustration have been widely accepted by statisticians, confusion is introduced by the neglect of mathematical conventions. To the non-mathematical statistician it seems more natural to erect height-ordinates vertically and to imagine a row of men standing on their feet. He should remember, however, that in the simple probability curve the heights were laid off horizontally on the axis of X and that if he wishes to have the height-ordinates vertical they must in both cases be laid off on the axis of Y. GALTON's "ogive" curve is obtained by tracing the integral curve on tissue-paper, looking at it from the back of the paper and turning it through 90°. But if this is done, the simple probability-curve must be treated in the same way. In respect to the proper choice of axes SEAVER's table, for example, is correct, BOWDITCH's tables and curves are not.

2. *The most probable value.* If x be taken to represent a set of values $x_1, x_2, \ldots, x_n$, the differences x_k-x_p, $(k=1,2,\ldots,n)$, can be considered as errors or detriments.[2]

The antecedent probability of any set of errors is proportional to

$$\phi(x_1-x_p)\ \phi(x_2-x_p)\ \ldots\ \phi(x_n-x_p).$$

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[1] GALTON, *Statistics by intercomparison*, Phil. Mag., 1875 (4) XLIX 33.

[2] GAUSS, Theoria combinationis observationum, I, 6.

That value of x_p which renders this product a maximum is *a priori* the most probable value. This requires that

$$0 = \frac{1}{\phi(x_1 - x_p)} \frac{d\phi(x_1 - x_p)}{d(x_p)} + \frac{1}{\phi(x_2 - x_p)} \frac{d\phi(x_2 - x_p)}{d(x_p)} + \ldots + \frac{1}{\phi(x_n - x_p)} \frac{d\phi(x_n - x_p)}{d(x_p)},$$

which is the equation of condition for the determination of x_p.[1]

This equation is used in two ways: 1. to determine the law of probability required for an assumed most probable value; 2. to determine the most probable value for an assumed law of probability. In either case an arbitrary assumption must be made, as Gauss clearly recognized.[2] If the average be assumed as the most probable value, then, with the usual additional assumptions,

$$\phi(v) = \frac{h}{\sqrt{\pi}} e^{-h^2 v^2} \tag{11}$$

where $v_k = x_k - A, \quad (k = 1, 2, \ldots, n)$

and $A = \frac{\Sigma x}{n}$.

If another mean, e. g. the median or the geometric mean, be assumed as the most probable value, the law of frequency takes a different form.

It has been customary to regard the assumption of the arithmetical mean and the exponential law of error therefrom deduced, as practically verified, although theoretically not correct.[3]

Since the fundamental supposition of symmetry of the probability-curve is quite unjustifiable for psychological measurements, and since the theory of means has been treated by Gauss independently of any definite law of error, it is justifiable to omit all further consideration of this treatment of the most probable value, although Gauss's earlier treatment is followed by most of the text-books on the adjustment of measurements.[4]

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[1] Gauss, Theoria motus corp. coel., II, 3, 177.

[2] Gauss, Theoria motus corp. coel., II, 3, 177.
Gauss, *Anzeige*, Gött. gel. Anz., 1821 Feb. 26; Werke, IV 98.

[3] Bertrand, Calcul des probabilités, 183, Paris 1889.

[4] Encke, *Ueber d. Methode d. kleinsten Quadrate*, Berliner Astr. Jahrb., 1834 249.
Chauvenet, Manual of Spherical and Practical Astronomy, 469, Phila., 1864.
Helmert, Die Ausgleichungsrechnung, Leipzig 1872.
Meyer, Vorlesungen ü. Wahrscheinlichkeitsrechnung, 243, Leipzig 1879.
Merriman, Method of Least Squares, New York 1894.
Weinstein, Handbuch der physikal. Maassbestimmungen, I 54, Berlin 1886.

3. *Algebraic selection.* Means can be classed according to the way in which they are computed.

a. Combinatory means.[1] These are

$$F_1(x)=\sqrt[1]{\frac{\Sigma x}{n}}$$

$$F_2(x)=\sqrt[2]{\frac{1.2.\ \Sigma_{\alpha,\beta}\ x_\alpha x_\beta}{n(n-1)}}$$

$$\vdots \qquad \vdots$$

$$F_r(x)=\sqrt[r]{\frac{1.2.\ .\ .\ .\ r.\ \Sigma_{\alpha,\beta,\ .\ .\ .,\rho}\ x_\alpha x_\beta\ .\ .\ .\ x_\rho}{n(n-1)\ .\ .\ .\ (n-r)}}$$

$$\vdots \qquad \vdots$$

$$F_n(x)=\sqrt[n]{x_1 x_2\ .\ .\ .\ x_n}.$$

Of these means only two have come into use, namely, the arithmetic mean

$$F_1(x)=\frac{x_1+x_2+\ .\ .\ .\ +x_n}{n}=A.$$

and the geometric mean

$$F_n(n)=\sqrt[n]{x_1 x_2\ .\ .\ .\ x_n}=G.$$

b. Power means. These are

$$p_1(x)=\sqrt[1]{\frac{\Sigma x^1}{n}}$$

$$p_2(x)=\sqrt[2]{\frac{\Sigma x^2}{n}}$$

$$\vdots \qquad \vdots$$

$$p_n(x)=\sqrt[n]{\frac{\Sigma x^n}{n}}.$$

[1] SCHEIBNER, *Ueber Mittelwerthe,* Ber. d. math.-phys. Cl. d. könig. sächs. Ges. d. Wiss., 1873 XXV 562.

FECHNER, *Ueber d. Ausgangswerth d. kl. Abweichungssumme,* Abhandl. d. math.-phys. Cl. d. könig. sächs. Ges. d. Wiss., 1878 XI 1(76).

By analogy we might put

$$p_0(x)=\sqrt[0]{\frac{\Sigma x^0}{n}};$$

but since $x^0=1$, then $p_0(x)=\sqrt[0]{1}=1^{\frac{1}{0}}=1^\infty$, which represents an indeterminate quantity[1] between 0 and ∞. This can be taken to express the fact that the median $M=p_0(x)$ is not dependent on the numerical values of $x_1, x_2, \ldots, x_n$.

The value

$$p_1(x)=\frac{x_1+x_2+\ \ldots\ +x_n}{n}=A$$

is the arithmetic mean. When x represents the errors from an average, their mean

$$p_2(x)=\sqrt[2]{\frac{x_1^2+x_2^2+\ \ldots\ +x_n^2}{n}}=m$$

is the mean-square-error as used in the method of least squares. The quartic mean $p_4(x)$ has also been used in the calculation of precision.[2]

4. *Selection to minimize a function of the variations.* Means have been so selected as to make

$$\frac{d(f(v))}{dR}=0,$$

where

$$v_k=x_k-R, \qquad (k=1, 2, \ldots, n).$$

For $f(v)=\Sigma(v)$, we have[3]

[1] Cauchy, Cours d'analyse algebr., 69, Paris 1821.

[2] Gauss, Theoria combinationis observationum, I, 11.

[3] Boscovich, De littera expeditione ad dimetiendos duos meridiani gradus, Romae 1755. (I have not seen this work. It is described in Todhunter, Hist. Theories Attract., I 305, 332, Lond. 1873.)

Boscovich, *De recentissimus graduum dimensionibus*, Philosophia recentior a B. Stay, II 420, Romae 1760. (I have not seen this work. It is described in Todhunter, Hist. Theories Attract., I 321, Lond. 1873.)

Laplace, *Mém. sur la prob.*, Mém. de math. et de phys. par divers savans, Acad. Paris, 1774 VI 621 (635).

Bernoulli, *Milieu*, Encyclopédie méthodique, Math., II 404, Paris 1785: Dict. encycl. d. math., Paris 1789.

Laplace, *Sur les degrés mesurés des méridiens*, Hist. Acad. Sci. Paris 1789, Mém. de math., 18.

Laplace, Mécanique celeste, III 40, Paris 1804; Oeuvres, II 144.

Laplace, Théorie anal. des prob., Suppl. 2, §2.

Fechner, *Ueber d. Ausgangswerth d. kl. Abweichungssumme*, Abhandl. d. math.-phys. Cl. d. König. sächs. Ges. d. Wiss, 1878 XI 1.

Glaisher, *On the law of facility of errors of observations*, Mem. Roy. Astr. Soc. Lond., 1872 XXXIX 75 (123).

$$\frac{d\Sigma(v)}{dR}=0,$$

and

$$R_0=M.$$

For $f(v)=\Sigma v^2$, or, what amounts to the same thing, for

$$f(v)=\sqrt[2]{\frac{\Sigma v^2}{n}}=m,$$

we have[1]

$$R_1=\frac{x_1+x_2+\quad.\quad.\quad.\quad+x_n}{n}=A$$

AVERAGE.

Average and centroid. The average is defined as

$$\mathrm{A}=\frac{x_1+x_2+\quad.\quad.\quad.\quad+x_n}{n}.$$

If m_1, m_2, . . ., m_r denote the number of times the values x_1, x_2, . . ., x_r occur respectively, then

$$\mathrm{A}=\frac{x_1+x_2+\quad.\quad.\quad.\quad+x_n}{n}=\frac{m_1x_1+m_2x_2+\quad.\quad.\quad.\quad+m_rx_r}{m_1+m_2+\quad.\quad.\quad.\quad+m_r}$$
$$=\frac{\Sigma mx}{\Sigma m}.$$

The average thus corresponds to the centroid of a system of parallel forces.

If the results are so numerous that the values of x can be treated as continuous and $\phi(x)dx$ can be substituted for m, the average can be substituted, with a small error ϵ, for the abscissa of the centre of gravity. Thus

$$\mathrm{A}=\frac{\int_{x_1}^{x_n}x\phi(x)dx}{\int_{x_1}^{x_n}\phi(x)dx}\pm\epsilon,$$

which in consideration of (2) and (4) becomes

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[1] LEGENDRE, Nouv. méthodes pour la détermination des orbites des comètes, VIII, Paris 1805.

MERRIMAN, *List of writings relating to the method of least squares*, Trans. Conn. Acad., 1877 IV 151.

$$A = \int_{x_1}^{x_n} x\phi(x)dx \pm \epsilon.$$

If $\phi(x)$ is unknown and the number of results is large, the average A represents the centroid-abscissa x_ξ with a degree of certainty and within limits determined by POISSON.[1] For a large number of results

$$A = x_\xi \pm \frac{2\gamma\sqrt{h}}{\sqrt{n}}, \tag{12}$$

with a probability of

$$\Phi(\gamma) = \frac{2}{\sqrt{\pi}} \int_0^\gamma e^{-t^2} dt.$$

where h is a quantity derived from the means of the first and second powers of the errors but is not amenable to practical calculation except for known $\phi(x)$.

Various other considerations bearing on the relation of the average to the centroid and to the individual results,[2] although necessary to a just appreciation of these relations, cannot be touched here. The assertion that the use of the average is the mean supposition of all possible suppositions as to the mode of obtaining value,[3] in addition to its questionable character,[4] rests on the assumption of symmetrical probability which cannot be admitted here.

Precision of the average. The calculation of the mean variation, the mean-square-error, the probable error, the constant of precision, etc., are to be found in the numerous works on measurement. They generally start with the assumption of a symmetrical curve of probability and pass over assymmetrical curves as being symmetri-

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[1] POISSON, Recherches sur la probabilité des jugements, ch. IV, Paris 1837.

[2] LAGRANGE, *Mém. sur l'utilité de la méthode de prendre le milieu entre les résultats de plusieurs observations*, Miscell. Taurinensia, 1770–1773, V (math.) 167; Oeuvres, II 171.

ENCKE, *Ueber d. Anwendung d. Wahrscheinlichkeits-Rechnung auf Beobachtungen*, Berliner Astr. Jahrb. f. 1853, 310.

PIZZETTI, *Sopra una generalizzazione del principo della media aritmetica*, Atti d. R. Accad. dei Lincei, Rend., 1889 (4) V_1 186.

[3] DE MORGAN, *On the theory of errors of observation*, Trans. Camb. Phil. Soc., X 409 (416).

[4] GLAISHER, *On the law of facility of errors of observation*, Mem. Roy. Astr. Soc. Lond., 1872 XXXIX 75 (90).

cal curves with systematic errors. An elementary treatment without this assumption is given by BERTRAND.[1]

Numerical error of the average. The computation of the average involves a decision on the number of decimal places to be retained in the observed values and in the mean after division.

The limitation of the number of decimal places used in writing a result introduces an error into each result. Each result differs from the truth by not more than one half-unit in the last place; its mean error is $\frac{1}{4}$ of a unit in the last place, or 0.25β where β denotes 1 unit in the last place.

When n results are added to form an average, the mean error of the sum will be $0.25\beta\sqrt{n}$. The average itself will have a mean error of $\frac{0.25}{\sqrt{n}}\beta$.

Although this supposition is the usual one for numerical work, it is not strictly true. Such a treatment of the mean error is valid only when the law of frequency is expressed by (11). The error thus introduced is, however, negligible.

Since it is understood that the last decimal place is subject to a mean error of 0.25β, the extra decimals obtained in calculating the average of 100 results may be retained to one place beyond the original results.

Even when the number of results is less than 100, the retention of one place further introduces less error than rounding off to a places. Thus for 25 results the mean error of the a place is 0.05β, and of the $a+1$ place is $0.50\beta'$. If we round off to the a place on a supposition of a mean error of 0.25β in the usual way, the uncertainty of the a place is increased.

Given the set of 9 values 213, 215, 213, 210, 212, 214, 215, 210, 212. The mean numerical error of each value is 0.25β. The mean error of the average will be

$$\frac{0.25\beta}{\sqrt{9}}=0.08\beta, \text{ or } 0.83\beta'.$$

The average is

$$\frac{1914}{9}=212.666\ \ldots\ldots$$

which is subject to a mean error of 0.08β in the first place before the point or $0.8\beta'$ in the first decimal place. If we round off to 213,

[1] BERTRAND, Calcul des probabilités, ch. X, Paris 1889.

we introduce an uncertainty corresponding to a mean error of 0.25β in the whole numbers, whereas by retaining the 6, the uncertainty corresponds to only 0.08β in the unit-place or to $0.8\beta'$ in the first decimal place, being a gain corresponding to 0.17β. Rounding off to 212.7 adds an uncertainty of $0.25\beta'$ in the first decimal place, giving a total of $0.83\beta'+0.25\beta'=1.08\beta'$ for that place or 0.108β for unit-place, being a loss corresponding to 0.028. Since under any circumstances the loss would have to be 0.025β, the writing of 212.67 has practically no advantage over 212.7.

The case is different when the uncertainty of the values is not due simply to the omission of decimal places. Let the measure of the uncertainty of a value x be denoted by $\pm\,\Delta x$. Then the uncertainty of the average of n results will be given by

$$\frac{\pm\,\Delta x}{\sqrt{n}}.$$

The mean error is the most convenient measure of uncertainty.

Under very favorable conditions the record of the Hipp chronoscope[1] is liable to a mean error of $\Delta x=1.5^\sigma$. The average of 9 records is reliable to 0.5^σ. To obtain a result numerically precise to 1^σ, i. e. with a mean error of 0.25^σ, it would be necessary to have 36 original records.

Thus, if a body, e. g. a control-hammer, were known to fall with perfect constancy, 36 records with the chronoscrope would be required to determine its time of fall to 1^σ.

Dependence on characteristic variations. The result of a set of direct measurements is stated to be $A\pm d$, $A\pm m$ or $A\pm r$. The quantity d is the mean variation or mean error, m is the mean-square-error and r is the half probable variation or probable error. These characteristic variations are determined from the formulas

$$d=\frac{(v_1)+(v_2)+\;.\;\;.\;\;.\;+(v_n)}{\sqrt{n(n-1)}}\left(1\pm\frac{0.756}{\sqrt{n-1}}\right)$$

$$m=\sqrt{\frac{v_1^2+v_2^2+\;.\;\;.\;\;.\;+v_n^2}{n-1}}\left(1\pm\frac{0.708}{\sqrt{n-1}}\right)$$

$$r=0.674\sqrt{\frac{v_1^2+v_2^2+\;.\;\;.\;\;.\;+v_n^2}{n-1}}\left(1\pm\frac{0.708}{\sqrt{n-1}}\right)$$

In these formulas the signs $\pm$ have the meanings usually given them in works on adjustment.

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[1] Külpe and Kirschmann, *Ein neuer Apparat zum Controle zeitmessender Instrumente*, Phil. Stud., 1892 VII 145.

These results indicate the range within which any other single measurement under the same circumstances may be expected to differ from A with a given degree of probability. Thus, we can wager 1 to 1 that a repetition of the measurement under the same conditions will give a result differing from A by not more than r.

In the final statement of the average it is unnecessary and misleading to use more figures than would be justified by the characteristic variations.

Thus if a set of 25 measurements on reaction-time gives an average of 0.2346 sec. with a probable error of 0.012 sec., the second figure, 3, of the result is uncertain to the extent of more than ± 1 unit. The third figure, 4, of the result is uncertain to the extent of ± 12 units, and the last figure, 6, to the extent of ± 120 units. As figures when rounded-off are understood to be uncertain to the extent of a mean error of ± 0.25 unit in the last place, the statement that the result is 0.23 sec. is somewhat less reliable than the figures themselves indicate and the statement that the result is 0.235 or 0.2346 is quite misleading. The usual method, whereby the average is given to the last place justified by the compution, while the amount of d, m or r is independently stated, is justifiable or not according to the purpose in hand. When the purpose is simply the determination of an average, there can be no ground for affixing meaningless decimal places; the average should not be stated further than the first place rendered insecure by the characteristic variation.

Median.

Median and middle value. The median is that value which is obtained by counting off an equal number from each end of the series of results arranged singly according to size. If $p_1, p_2, \ldots, p_n$ represent the relative frequencies of $x_1, x_2, \ldots, x_n$, then

$$\sum_{x_1}^{M} p = \sum_{M}^{x_n} p.$$

The values $x_1, x_2, \ldots, x_n$ have all an equal influence in the determination of x_m. Each quantity x_k is either above or below x_m; how far above is not regarded. Those above x_m might be called positive results and those below x_m might be called negative. Thus we might put

$$\sum_{x_m}^{x_n} p = r \qquad \sum_{x_1}^{x_m} p = s$$

and $$r+s=\mu$$

or $$r=s=\frac{\mu}{2}.$$

The relation between M and x_m can be determined by BERNOULLI's theorem. When μ is large,

$$M=x_m \pm \gamma r\sqrt{\frac{2}{\mu}} \tag{13}$$

with a probability of

$$P=\frac{2}{\sqrt{\pi}}\int_0^{\gamma} e^{-t^2}\,dt+\frac{\sqrt{\mu}\,.\,e^{-\gamma^2}}{r\sqrt{2\pi}}$$

The values of γ are to be determined from the usual table for

$$\Phi(\gamma)=\frac{2}{\sqrt{\pi}}\int_0^{\gamma} e^{-t^2}\,dt.$$

Computation of the median. The median is defined as that value which occupies the position given by

$$\frac{x_1^{\circ}+x_2^{\circ}+\ .\ \ .\ \ .+x_n^{\circ}+1}{2}=\frac{n+1}{2}$$

in the series of values $x_1, x_2, \ldots, x_n$ taken in order of size from the smallest to the largest and from the largest to the smallest.

Let the number of occurrences of each value of x be denoted by $m_a, m_b, \ldots, m_r$. The series of differing values $x_1, x_2, \ldots, x_r$, finitely expressed, can be regarded as having arisen from the series $x_1, x_2, \ldots, x_n$ expressed each to an infinite number of decimals by rounding-off all the decimals to the a place. In the a place the set $x_1, x_2, \ldots, x_a$ will all agree and can be expressed by $m_a\,x_a$. Likewise we have the sets $m_b\,x_b, \ldots, m_r\,x_r$.

When these sets are arranged in order of size

$$m_a\,x_a,\ m_b\,x_b,\ \ldots,\ m_{l-1}\,x_{l-1},\ m_l\,x_l,\ m_{l+1}\,x_{l+1},\ \ldots,\ m_r\,x_r$$

the set containing the median will be $m_l\,x_l$ where

$$(m_a+m_b+\ .\ \ .\ \ .\ +m_{l-1})-(m_{l+1}+m_{l+2}+\ .\ \ .\ \ .\ +m_r)<m_l.$$

The set containing the median is not necessarily the middle set.

The median will be one of the m_l values which have been rounded

off to the same value x_l. Each value x_l represents some value $x_l \pm 0.5\beta$ where β indicates 1 unit of the order α.

When

$$(m_a+m_b+\ .\ .\ .\ +m_{l-1})-(m_{l+1}+m_{l+2}+\ .\ .\ .\ +m_r)=-c,$$

the median value occupies a place among the m_l values given by c where

$$(m_a+m_b+\ .\ .\ .\ +m_{l-1})+c+\frac{m_l-c}{2}=\frac{m_l-c}{2}+(m_{l+1}+m_{l+2}+\ .\ .\ .\ +m_r).$$

The median is thus not the middle value of the group m_l but of the group m_l-c.

If the whole interval from which the values of x_l were derived be denoted by S, then the position of M within the interval S will be given by

$$M=x_l+\frac{c}{2m_l}S$$

If the extreme values for x_l be

$$x_l'=x_l-0.5\beta$$
$$x_l''=x_l+0.5\beta$$

where β is 1 unit of the last place, then

$$M=x_l+\frac{c}{2m_l}\beta$$

For the results given on p. 17, 210 is the smallest value, 210 the next, 212 the next, etc. As there are 9 values, the median will be the $\left(\frac{9+1}{2}=5\right)$th value from the smallest; this is 213. The largest value is 215, the next 215, the next 214, etc. The 5th from the largest is 213.

The 4th value from the largest is also 213^σ. Thus $x_l=213^\sigma$, $m_l=2$, $c=+1$ and $\beta=1^\sigma$. Consequently

$$M=213^\sigma+\tfrac{1}{4}1^\sigma=213.25^\sigma.$$

The following example of calculating the median is given by Fechner.[1] As a historical interest is present, the rather naïve method of using decimals is left untouched.

"Take the case where the result runs as follows :

result	1	2	3	4	5
number of times it has occurred	2	5	16	10	7

[1] Fechner, *Ueber d. Ausgangswerth d. kleinsten Abweichungssumme*, Abhandl. d. math.-phys. Cl. d. k. sächs. Ges. d. Wiss., 1878 XI 1 (19).

"The total number n is here 40, and the $\frac{n+1}{2}$., i. e. the $20\frac{1}{2}$. value counted from either left or right end of the series cuts into the number 16; thus the median is to be sought by interpolation in the series of the 16 results giving the value 3, but as the 20th, not as the $20\frac{1}{2}$th. The limits of this series of 3s are 2.5 and 3.5. Counting from the left side we have 7 values up to the limit 2.5 of the series and 13 more are needed to make the 20th value which falls among the 3s; thus according to the simplest principle of interpolation we have to take from the limit 2.5 upward still $\frac{13}{16}=0.8125$ of this interval in order to reach M, making $M=2.5+0.8125=3.3125$. Going from the right hand end, we have 17 values up to the limit 3.5 of the series, and lack still 3 of the 20, which fall in the series. Thus we have to subtract from the limit $\frac{3}{16}=0.1875$ of the interval in order to reach M, which gives $3.5-0.1875=3.3125$ as before."

Numerical error of the median. The mean numerical error of a single value being 0.25β, that for m_t values will be

$$\frac{0.25}{\sqrt{m_t}}\beta.$$

In the foregoing example where $m_t=2$, the mean error of the result 213^σ is 0.18β and of the extended result 213.3^σ is $1.8\beta'$, where $\beta=1^\sigma$ and $\beta'=0.1^\sigma$.

As a slightly different example take the values 44, 51, 46, 50, 47, 49, 47, 45, 48, 50. The median will be the $\left(\frac{10+1}{2}\right)$th or $5\frac{1}{2}$th value. The fifth from the smallest is 47; the fifth from the largest is 48; the $5\frac{1}{2}$th will lie between the two. As there is no reason to prefer one extreme of the interval $48-47$ to the other, the simplest method is to take $M=47.5$. The numerical uncertainty of 48 is represented by a mean error of 0.25β; that of 47, derived from two results by $\frac{0.25\beta}{\sqrt{2}}=0.18\beta$. The mean error of their sum will be

$$\sqrt{(0.25)^2+(0.18)^2}\beta=0.31\beta;$$

and of their average $\qquad \frac{0.31\beta}{2}=0.16\beta.$

The numerical mean error for the median in this example is thus 0.16β for the unit-place.

The decimal place, already uncertain by a mean error of $0.25\beta'$, becomes uncertain to the extent of a mean error of $1.85\beta'$. Although 47.5 is less uncertain than 47 or 48, it has nevertheless quite a numerical uncertainty.

The numerical uncertainty of the average of the ten results would be indicated by a mean error of $\frac{0.25\beta}{\sqrt{10}}=0.08\beta$ for the unit-place or $0.8\beta'$ for the first decimal. The median is thus at a disadvantage numerically.

As the results gather more around a middle value in more accurate work, more values will coincide with the median, m_i will become larger and the numerical error for a given number of results will become less. The numerical error of the average will remain the same.

As mentioned on pages 2 and 17 the influence of $\phi(x)$ on the numerical error is negligible. Thus in calculating the numerical error of the median, as long as the unit of number does not exceed the size of the mean error, we can safely suppose the values x_i to have arisen by rounding-off equally frequent values throughout β.

Dependence of accuracy on the number of results. GAUSS has deduced an expression for the accuracy of the mean value of any power of a variation as depending on the number of variations.[1] With slight changes the results can be stated as follows on the supposition of (11). Let x_κ be the mean as determined from the κ powers of the n observations. Then with not too small numbers of results the probable uncertainty, in the same sense as the probable error, for $x_0=M$ determined from $x_1^0, x_2^0, \ldots, x_n$ is $\pm\frac{0.752}{\sqrt{n-1}}$; for $x_1=A$ determined from $x_1^1, x_2^1, \ldots, x_n^1$ it is $\pm\frac{0.510}{\sqrt{n-1}}$.

Other things being equal, it is necessary to take 249 observations to gain the same accuracy for the median as is given by 114 observations for the average.

Dependence on characteristic variations. As noted on p. ? the median is that representative value which corresponds to a minimum for the sum of the absolute values of the first powers of the variations. The mean variation from the median will bear to the median a relation similar to that which the mean-square-error bears to the arithmetic mean. The median will thus be stated as

$$M\pm a,\ M\pm l \text{ or } M\pm s, \text{ where}$$

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[1] GAUSS, *Bestimmung d. Genauigkeit d. Beobachtungen*, Zt. f. Astr., 1816 I 185; Werke, IV 109.

LIPSCHITZ, *Sur la combinaison des observations*, C. R. Acad. Paris, 1890 CXI 163.

$$a=\frac{\Sigma(x-M)}{\sqrt{n(n-1)}}\left(1\pm\frac{0.756}{\sqrt{n-1}}\right).$$

$$l=\sqrt{\frac{\Sigma(x-M)^2}{n-1}}\left(1\pm\frac{0.708}{\sqrt{n-1}}\right)$$

$$s = 0.674\ l.$$

The number of significant figures to be retained in stating the median is regulated in the same way as for the average. For the example given on p. 17, the median is calculated to be 213.3^σ; the mean variation is 1.6^σ. The last figure is uncertain by at least 1.6^σ and cannot justifiably be used for final statement. Even the last figure in 213^σ is slightly uncertain.

In FECHNER's example on p. 21 the mean variation is 1.14. This makes even the whole number 3 rather uncertain and for final statement renders utterly valueless the four-place decimal.

In the example given on p. 22, the mean variation of the individual results from the median is 2.0. Whence it follows that for the mere statement of a representative result, the decimal place is totally worthless and even the unit-place is unreliable. The mean variation from the average 47.7 is also 2.0. The conclusion is the same. Thus the average in a case of this kind has no advantage whatever.

As the mean variation becomes less, owing to better agreement of the results, the numerical mean error of the median will also decrease, whereas that of the average will remain the same. Thus the numerical advantage of the average is valueless, for a final statement, when the mean variation is large, and this advantage itself is lost as the mean variation decreases.

The labor of obtaining the extra decimal is thus not justified when the results disagree to such an extent. By a preliminary estimate or computation or by a cursory glance at the values themselves it is generally possible to determine the number of places required and thus to adjust the amount of the labor to the worth of the result.

In the consideration of the characteristic variations both for the median and for the average, I have, for the sake of using GAUSS's deductions, made the supposition that the law of error is

$$\phi(v)=\frac{h}{\sqrt{\pi}}e^{-h^2v^2}. \tag{11}$$

Labor of computation. There is one important property of the median which can be understood only after practical acquaintance with it, namely, its economy. Suppose the original results of the

example already used to be set down in a line or a column: 213, 215, 213, 210, 212, 214, 215, 210, 212. Let the higher numbers be marked off successively by a small check, thus 2ĭ5, till 5 have been checked. This is the median. After a little practice this can be done for 10 or 25 values with unexpected rapidity. The result is rapidly verified by checking off the numbers from the smallest upward. The determination of the average requires the addition of 9 figures and the division of the result by 9. Even in this example where the first two figures can be neglected and the whole work can be done mentally, yet the time required is much longer.

In all examples mentioned, owing to the size of the mean variation there would have been no gain in computing the average, whereas the additional labor would have been a decided loss. When it is remembered that saving in labor in computation means additional opportunity for obtaining results, it is justifiable to claim that the most economical method of computation should be employed.

In the typical examples given the additional uncertainty of the median is negligible in comparison with the characteristic variations. When this is not the case, it is a question whether to obtain double the number of results for the median or to perform the additional computation required by the average, in order to obtain the mean with a given precision.

Median error. On the assumption of (11) the probable error should be nearly the same whether determined by BESSEL's formula

$$r_1 = 0.674 \sqrt{\frac{v_1^2 + v_2^2 + \ . \ \ . \ \ . + v_n^2}{n-1}}$$

or by PETERS's formula

$$r_2 = 0.845 \ \frac{(v_1) + (v_2) + \ . \ \ . \ \ . + (v_n)}{\sqrt{n\,(n-1)}}$$

or by counting off in order of size till the middle error is reached. The probable error is thus in the last case the median error. In the same way that the mean error for the median corresponds to the mean-square-error for the average, so the median error for the median would correspond to the mean error for the average. In a similar manner the median error for the median can be compared with the probable error for the median, in order to test the validity of the assumption mentioned.

Other discussions on the median. In addition to the productions cited elsewhere, several other articles containing accounts of or refer-

ences to the median have been consulted. Those whose titles I have noted down are by COURNOT,[1] EDGEWORTH,[2] TURNER,[3] SCRIPTURE,[4] VENN.[5]

WEIGHT AND INFLUENCE.

In forming a direct average each measurement in a given set of n measurements has an influence of

$$f_1=\frac{x_1}{n}\ ;\ f_2=\frac{x_2}{n}\ ;\ .\ .\ .\ ;\ f_n=\frac{x_n}{n}$$

on the result. The influence of a quantity is thus directly proportional to its numerical value. The numerical values x_1, x_2, . . ., x_n can thus be called the relative influences of the 1, 2, . . ., nth measurements.

In combining results from different sets of measurements it is not always desirable for them to influence the average in direct proportion to their face-values. This has led to the use of a system of multipliers, called weights, by which the influence of a quantity is modified. The various results are each multiplied by a coefficient $p_1, p_2,$ . . ., p_n such that $\Sigma p=n$. The average of the weighted results will be the "weighted mean,"

$$A_p=\frac{p_1x_1+p_2x_2+\ .\ .\ .\ +p_nx_n}{p_1+p_2+\ .\ .\ .\ +p_n} \tag{14}$$

The fact that in concrete cases Σp is not equal to n arises from a tacit division of both numerator in (14) by the same number.

The weighted mean agrees with the direct mean only when

$$p_1=p_2=\ .\ .\ .\ =p_n. \tag{15}$$

It is a very natural step to apply this concept to individual measurements; some measurements are naturally better than others. But when all measurements have been made apparently with equal care and there is no reason to prefer one to another, it may be said that there is no *a priori* reason for weighting one different from

[1] COURNOT, Exposition de la théorie des chances et des probabilités, 120, Paris 1843.

[2] EDGEWORTH, *On discordant observations*, Phil. Mag., 1887 (5) XXIII 364.

EDGEWORTH, *New method of reducing observations*, Phil. Mag., 1887 (5) XXIV 222.

EDGEWORTH, *Empirical proof of the law of error*, Phil Mag, 1887 (5) XXIV 330.

EDGEWORTH, *Discussion on Dr.* VENN'S *paper*, Jour. Roy. Statist. Soc. Lond., 1891 LIV 453.

[3] TURNER, *On Mr.* EDGEWORTH'S *method of reducing observations*, Phil. Mag., 1887 (5) XXIV 466.

[4] SCRIPTURE, *On the adjustment of simple psychological measurements*, Psych. Rev., 1894 I 281.

[5] VENN, *On averages*, Jour. Roy. Statist. Soc. Lond, 1891 LIV 429.

another. The simplest assumption is the purely arbitrary one that all are of equal weight.

This gives a very good result if the values of x run along very regularly and close together. No dissatisfaction is felt as long as the individual variations fall within the limits.

$$-l < v_k < +l$$

or[1]

$$v_k < (l)$$

where

$$v_k = x_k - A, \quad (k = 1, 2, \ldots, n), \tag{16}$$

l not being considered a large quantity. But if some very large value x_r occurs so that

$$(v_r) > (l),$$

the natural supposition is that x_r is not so reliable as the other values of x. Sometimes it is rejected, i. e. the weight $p_r = 0$ is attached to it while $p_1 = p_2 = \ldots = p_{r-1} = p_{r+1} = \ldots = p_n = 1$. Sometimes it receives a weight $p_r = \frac{1}{2}$, or $p_r = \frac{1}{3}$, while the others receive $p = 1$, in a purely arbitrary fashion.

To know when to reject values it is necessary to assign some value to l. This has led to various criteria for rejection, the best known of which are those of PEIRCE,[2] CHAUVENET[3] and STONE.[4]

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[1] (x) is used for abs x, or x taken without regard to sign.

[2] PEIRCE, *Criterion for the rejection of doubtful observations*, Astr. Jour. (Gould), 1852 II 161.

GOULD, *Report . . . containing directions and tables for the use of* PEIRCE'S *criterion*, U. S. Coast Surv., Rept. 1854, 131*.

GOULD, *On* PEIRCE'S *criterion for the rejection of doubtful observations, with tables*, Astr. Jour. (Gould), 1855 IV 81.

AIRY, *Letter from . . .* [remarks on PEIRCE'S criterion], Astr. Jour. (Gould), 1856 IV 137.

WINLOCK, *On Prof.* AIRY'S *objection to* PEIRCE'S *criterion*, Astr. Jour. (Gould), 1856 IV 145.

NEWCOMB, *A generalized theory of the combination of observations so as to obtain the best results*, Am. Jour. Math , 1886 VIII 343. (The note on p. 344 contains two striking deductions)

[3] CHAUVENET, Manual of Spherical and Practical Astronomy, 564, Phila. 1864.

[4] STONE, *On the rejection of discordant observations*, Month. Not. Roy. Astr. Soc. Lond , 1868 XXVIII 165.

GLAISHER, *On the rejection of discordant observations*, Month. Not. Roy. Astr. Soc. Lond., 1873 XXXIII 391.

STONE, *On the rejection of discordant observations*, Month. Not. Roy. Astr. Soc. Lond., 1873 XXXIV 9.

GLAISHER, *Note on a paper by Mr.* STONE . . . , Month. Not. Roy. Astr. Soc. Lond., 1874 XXXIV 251.

STONE, *Note on a discussion . . .* , Month. Not. Roy. Astr. Soc. Lond., 1875 XXXV 107.

The criteria for rejection have never proven satisfactory. The general sentiment seems to be that the rejection of an honestly made observation simply because it differs largely from the expected value amounts to an attempt to make the work appear more accurate than it is.[1] The rejection of observations by calculation of the average first with it and then without it, is said to be like what happens in war when two detachments of the same army meet in the dark and fire into each other, each supposing the other to belong to the common enemy.[2]

The very questionable justification for any rejection of results on account of their divergence has led to various systems of weights.[3] The main objections to these systems are 1. the assumption of the validity of SIMPSON's law[4] $\phi(-v_k) = \phi(+v_k)$, where v_k is defined as in (16) and ϕ indicates the relative number of times of occurrence of v_k; 2. the labor of computation which is often out of all proportion to the gain.[5]

The rejection of observations has been a troublesome question for psychologists. The results did not agree and absolutely refused to group themselves around the arithmetical mean. It was not a question of a single result differing from all the rest but of several results tending toward an extreme value. This led to a process of wholesale rejection[6] which reached a crisis in giving double sets of results, once

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[1] HALL, *Orbit of Iapetus*, 40, Astr. and Meteor. Obs. for 1882, U. S. Naval Obs., App. I., Wash. 1885.

NEWCOMB, *A generalized theory of the combination of observations so as to obtain the best result*, Am. Jour. Math., 1886 VIII 343 (345).

FAYE, *Sur certains points de la théorie des erreurs accidentelles*, C. R. Acad. Sci. Paris, 1888 CVI 783.

[2] DOOLITTLE, *The rejection of doubtful observations*, Wash. Bull. Philos. Soc., 1884 VI 152, in Smithsonian Misc. Coll., 1888 XXXIII.

[3] DE MORGAN, *Theory of probability*, Encyc. Metropol., II 456, Lond. 1847.

GLAISHER, *On the law of facility of errors of observation and on the method of least squares*, Mem. Roy. Astr. Soc. Lond., 1871 XXXIX Pt. I. 75 (103).

NEWCOMB, *A generalized theory of the combination of observations so as to obtain the best result*, Am. Jour. Math., 1886 VIII 343.

SMITH, *True average of observations*, Nature, 1888 XXXVII 464.

[4] SIMPSON. *An attempt to show the advantage arising by taking the mean*, Misc. Tracts, 64, Lond. 1757.

[5] EDGEWORTH, *Choice of means*, Phil. Mag., 1887 (5) XXIV 268 (271).

[6] EXNER, *Exper. Untersuch. d. einfachsten psych. Processe*, Arch. f. d. ges. Physiol. (Pflüger), 1873 VII 601 (613).

v. KRIES and AUERBACH, *Die Zeitdauer einfachster psychischer Vorgänge*, Arch. f. Physiol. (Du Bois-Reymond), 1877 297 (307).

without rejection and then with rejection,[1] and found its *reductio ad adsurdum* in making experiments in sets of 25 of which 20 were selected for the calculation of the average.[2]

All this has made it evident that, even if the arithmetic mean of the observations is justifiable in the physical sciences, it is not *a priori* the best representative value for psychological measurements.

The trouble lies in the fact that, because the average is the most plausible representative for certain kinds of differing quantities, it has been treated as the best one in all cases.[3] In the use of the average each individual quantity influences the result in direct proportion to its numerical value. The value $x_i = a$ contributes to the result $\frac{a}{b}$ times as much as the value $x_k = b$. This is unquestionably the correct method to pursue when the individuality of the quantity is of no account. If r cubic meters of soil must be removed for a railroad-cut, it makes little difference just how much each particular car of a train carries, provided the average is satisfactory. Each car counts not merely as an overloaded or an underloaded car, but as overloaded or underloaded to a definite extent.

This is not the case in most measurements. The measurements tend to group themselves around some mean value, and when a widely different value occurs it is looked upon with distrust. To use it in an average is to make it count, not as one single value above or below the mean, but as an individual counting for every unit of divergence. The more it differs, the more it counts.[4] For example, in a set of values 3, 4, 2, 2, 3, 10, it is evident from mere inspection that the grouping is around 3. The average is 4, because the one extreme value 10 contributed to the formation of the average as much as the four values 3, 2, 2, 3 put together. If we had 15 instead of 10, the influence of this extreme value would have been more than that of all the rest. In the particular case of measurements the more a value differs from the rest, the less we think of it; if it

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[1] BERGER, *Ueber d. Einfluss der Reizstärke auf d. Dauer einfacher psych. Vorgänge*, Phil. Stud., 1886 III 38 (61).

CATTELL, *Psychometrische Untersuchungen*, Phil. Stud., 1886 III 305 (317).

[2] JASTROW, *Studies from the Univ. of Wisconsin.* Am. Jour. Psych., 1892 IV 382 (413).

[3] DE MORGAN, *On the theory of errors of observation*, Trans. Camb. Phil. Soc., X 409 (416).

VENN, *On averages*, Jour. Roy. Statist. Soc. Lond., 1891 LIV 429.

[4] BOWDITCH, Note to LAPLACE'S Mécanique celeste, translated, vol. II, 434, Boston 1832.

differs too much, we think so little of it that we are tempted to throw it out altogether.

Instead of allowing the individual result to have an influence proportional to its value, why not let it enter into the formation of the mean as one individual differing from the mean regardless of the amount of the difference?

Thus, if the graduating class in college happens to contain one very tall man, the average height will be greater than the averages for other years. The tall man contributes to the representative value not simply as one man but with the influence of several men.

It would seem more natural that each individual should influence the representative value merely as one individual. If all the men were placed in order of height, the most natural representative would be the man in the middle, or the man who would be determined by counting off an equal number of individuals from both extremes. This is equivalent to determining M by the formula on p. 20; and the height of this middle man is the median height. If there happens to be a very tall man among them, a few millimeters more or less in his height will make a difference in the average, but as long as he remains taller than the middle man the median will remain the same.

Let the set of results be indicated by x_1°, x_2°, . . ., x_n°, where these letters are used merely as symbols for single quantities. Thus, if the measurements are the heights of a set of individuals, x_1° will designate a certain individual; if they are observations, each will designate an observation. In taking the average, we influence each individual quantity with a number indicating its numerical value. Thus

$$\frac{\Sigma x x^{\circ}}{n} = \frac{x_1 x_1^{\circ} + x_2 x_2^{\circ} + \ . \ \ . \ \ . + x_n x_n^{\circ}}{n} = A.$$

But let a tall man count as only one tall man regardless of just how tall he is, provided he is above the mean, and let a short man count likewise as one man. Since tall and short are only relative terms, there must be some value above which all the men are to be called tall and below which they are to be called short. This is the value that we have called the middle value. Each individual counts as one unit. In general terms,

$$\frac{x_1^{\circ} + x_2^{\circ} + \ . \ \ . \ \ . + x_n^{\circ}}{n} = \frac{\Sigma x^{\circ}}{n} = M^{\circ}.$$

Each value thus has the same influence.

The influence of an individual measurement can be defined as its relative effect in the formation of the mean; its weight is an arbi-

trary multiplier prefixed to its numerical value. The influence of a measurement in taking an average is thus the product of its numerical value by its weight.

The general formula (14) for the weighted mean becomes in the notation just used,

$$A=\frac{p_1x_1x_1^\circ+p_2x_2x_2^\circ+\ .\ .\ .\ +p_nx_nx_n^\circ}{p_1+p_2+\ .\ .\ .\ +p_n}.$$

The supposition of

$$p_1=p_2=\ .\ .\ .\ =p_n=1 \tag{17}$$

has, as just noted, not proved satisfactory, extreme values being less trustworthy than moderate ones.

It has been proposed, as an assumption more satisfactory than (17), that each quantity be weighted inversely as its numerical difference from the average, whereby a corrected average will be obtained.[1] Let

$$v_k=x_k-A,\quad (k=1,\,2,\,.\ .\ .,\,n),$$

then take $\frac{1}{v_k}$ as the weight of x_k, whereby

$$A_i=\frac{\frac{1}{v_1}x_1+\frac{1}{v_2}x_2+\ .\ .\ .\ +\frac{1}{v_n}x_n}{\frac{1}{v_1}+\frac{1}{v_2}+\ .\ .\ .\ +\frac{1}{v_n}}.$$

Since the average differs from the centroid on account of $n<\infty$, this corrected average A_i may be treated to a still further correction in the same way. This process, when repeated tends to one of the given measurements as the mean.[2]

What is here approached in a way so awkward as to preclude any but a theoretical interest, can be very simply stated.

Let each quantity have a weight inversely proportional to its difference from the middle value. The centroid of a series of observations thus weighted will coincide with the middle value within the limits of error necessitated by $n<\infty$.

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[1] ———, *Dissertation sur la recherche du milieu le plus probable*, Annales de math. (Gergonne), 1821 XII 181.

DE MORGAN, *Theory of prob.*, Encycl. Metropol., II 440, Lond. 1847.

GLAISHER, *On the rejection of discordant observations*, Month. Not. Roy. Astr. Soc. Lond., 1873 XXXIII 391.

STONE, *On the rejection of discordant observations*, Month. Not. Roy. Astr. Soc. Lond., 1873 XXXIV 9.

[2] GERGONNE, *Note*, Annales de math. (Gergonne), 1821 XII 204.

The case will be represented by a rod without weight having on it a number of particles $x_1, x_2, \ldots, x_n$ with masses inversely proportional to their distance from the point of suspension. The distance of any particle from the point of suspension a is $x-a$; its weight is $\frac{1}{x-a}$; its moment is 1. The point on which the rod will balance is thus determined by the condition that there shall be an equal number of particles on each side.[1] The center of gravity of this system of particles will thus be its middle particle for an uneven number, or a point between the two middle ones for an even number.

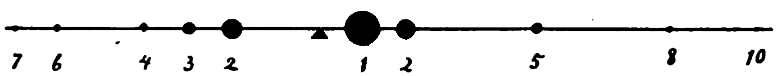

FIG. 4.

For a continuous mass governed by the same law, the centre of gravity x_ξ and the middle value x_m coincide.

From these considerations regarding influence and weight the following conclusions can be drawn:

The median represents the series of quantities in such a way that each quantity has an influence of unity.

The average represents the series of quantities in such a way that each quantity has an influence directly proportional to its numerical value.

The median is equal to the weighted mean where the weights are inversely proportional to the differences of the values from the mean.

The average is equal to the weighted mean where the weights are equal.

CHOICE OF MEANS.

The selection of representative values may take place under three different conditions: I. the results may be so numerous that the empirical frequency curve can be used as the probability curve; II. the results are few but the form of $\phi(x)$ has been determined; III. the results are few and $\phi(x)$ is unknown.

I. *Numerous results.* The fundamental difference between statistics and ordinary measurements lies in the number of measurements executed. Although the passage from one class to another is grad-

[1] WILSON, *Note on a special case of the most probable result of a number of observations,* Month. Not. Roy. Astr. Soc. Lond., 1878 XXXVIII 81.

ual, we can confine ourselves here to the extreme cases. When the results are so numerous that the curve of frequency can be plotted and can be regarded as identical with the curve of probability, every value for x and $\phi(x)$ is assumed as known. The representative values can be calculated from the formulas :
for the centroid

$$x_\xi = \frac{\Sigma\phi(x)x}{\Sigma\phi(x)}$$

for the median

$$\sum_{x_1}^{x_m=M}\phi(x) = \sum_{x_m=M}^{x_n}\phi(x)$$

and for x_k

$$\frac{d\phi(x)}{dx} = 0,$$

where $d^2\phi(x)/dx^2$ is $-$.

It is to be noted that there is often more than one value for x_k.

The allowable difference between the actual frequency $\frac{m_k}{m}$ and the probability $\phi(x)dx$ for each value of x can be determined by BERNOULLI's theorem.

When the object of the statistical measurements is merely the determination of the objects measured under a single set of conditions, the result is generally presented in the form of a curve of frequency; thus, all values and their weights being given, there is no need of a selection of any representative value. In scientific work, however, the purpose is generally to determine the change in the results as dependent on a change of conditions ; and the observed value is treated as a function of one of the conditions. Given $x=f(z)$ to determine x for each value of z where the measurements for each value of x are numerous. Such an example would be furnished by measuring 10 000 persons each year to determine the law of dependence of height on age. Although the curve of frequency of heights at each year could be made out, still, aside from the impracticability of so much labor in most cases, it would be impossible to give any intelligible expression to the law of relation. Some representative value or values must be picked out for each step of the change. Owing to the fact that x_ξ, x_m and x_k almost never coincide, it is very desirable that all three shall be given. There will thus be three curves,

$$x_\xi = f_1(z),$$
$$x_m = f_2(z),$$
$$x_k = f_3(z).$$

The changes in relative position of these values indicate changes in the character of the quantity measured.[1]

II. *Results not numerous but $\phi(x)$ known.* The law of frequency can be considered to be known: 1. when numerous results have been taken on previous occasions under the same circumstances whereby the law of frequency has been determined with the requisite accuracy; 2. when a knowledge of the circumstances indicates what the law must be.

In this connection it may be well to call attention to the fact that the statement of the law of error as

$$\phi(v)=\frac{h}{\sqrt{\pi}}e^{-h^2v^2} \tag{11}$$

rests (*a*) on the assumption that the average is the most probable value, (*b*) on the assumption that experience has shown such a law to be true, (*c*) on the fact that it is the limiting form for combinations of symmetrical frequency curves, or (*d*) on the simplicity of treatment thereby rendered possible. I have already pointed out that GAUSS clearly recognized and distinctly stated[2] (*a*) as an assumption, and that long experience has shown (*b*) not to be strictly justifiable. This law of error has done probably better service in astronomy than any other could have done and long familiarity with both assumptions has made them appear almost as axioms. WEINSTEIN,[3] who is careful to call attention to the fact that the assumption (*a*) is not an axiom, is mistaken in asserting that GAUSS regarded it as such. WEINSTEIN is also mistaken in supposing that SCHIAPARELLI[4] attempted to prove analytically that the average is the most probable result. SCHIAPARELLI showed that under certain assumptions the average is the most plausible mean, and stated that it becomes the most probable mean only when (11) is the law of error.

According to FERRERO[5] the utmost defence of the use of the arithmetic mean for all cases is that, when the observations are closely grouped, no mean will differ much from the arithmetic mean.

Apparently still more axiomatic is the law $\phi(-v)=\phi(+v)$. By

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[1] BOWDITCH, *Growth of children*, XXII Annual Rept. Mass. State Board of Health. 479 (495), Boston 1891.

[2] GAUSS, Theoria motus corp. cœl., II, 3, 177.

[3] WEINSTEIN, Physikalische Maassbestimmungen, I 46, Berlin 1886.

[4] SCHIAPARELLI, *Sur le principe de la moyenne arithmetique*, Astr. Nachr., 1876 LXXXVII 55.

[5] FERRERO, Esposizione del metodo dei minimi quadrati, Firenze 1876. (I take the statement from a review by PEIRCE, Am. Jour. Math., 1878 I 59.)

most writers it is so regarded. Nevertheless GAUSS makes the statement purely as a hypothesis[1] and LAPLACE gives a special paragraph to the consideration of unsymmetrical facility.[2]

Even if not assumed as an axiom the law is almost universally supposed to have been verified by experience. It is not in place here to consider whether it has been verified for astronomical measurements or not.[3] It has not been verified for psychological measurements. Since the errors of observation in astronomy are in part due to psychological causes, it seems likely that all astronomical records involving an observer would show some assymmetry.

The assumption, without proof, that this law always holds good and that all cases of assymmetry are cases of constant or systematic error, is purely arbitrary.

It is thus evident that many of the cases, supposed to belong in this section, really belong to the following one where $\phi(x)$ is unknown.

According as $\phi(x)$ is (A.) symmetrical or (B.) assymmetrical the treatment of the results and the selection of the representative value will be different.

A. *Symmetrical results.* When the curve of frequency is symmetrical, the ordinate of middle area and the ordinate of the centroid will be the axis of symmetry.[4] Thus $x_m = x_\xi$ and $M = A$ within the allowable limits of error corresponding to the required certainty.

If, according to usual experience, the extreme values occur less frequently than those between the extremes, x_h will in general be the same as x_m and x_ξ, and will be represented by M and A. The curve of probability may, however, have several maxima, none of which may fall at x_m.[5]

In a general fashion the law of frequency for physical, geodetical and astronomical measurements has been found to resemble (11). This law was not, however, originally established on the basis of experience, but was deduced as a necessary result of the arbitrary assumption that A is the most probable value.[6]

Although it has been approximately verified on many occasions, a closer examination shows considerable disagreement in the assymmet-

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1 GAUSS, Theoria combinationis observationum, I, 5.

2 LAPLACE, Théorie analyt. d. prob., 3. éd., 329, Paris 1820.

3 DE FOREST, *On an unsymmetrical probability curve*, Analyst 1882 IX 135, 142; 1883 X 1, 67 (71).

4 DE FOREST, *On unsymmetrical adjustments and their limits*, Analyst, 1880 VII 1.

5 EDGEWORTH, *Observations and statistics*, Trans. Camb. Phil. Soc., XIV 138 (161).

6 GAUSS, Theoria motus corp. coel., II, 3, 177.

rical position of x_i and in the undue number of extreme values. NEWCOMB even concludes that cases where it is fully valid are exceptional.[1]

In any case of symmetry, whether (11) is valid or not, the median and the average will be theoretically the same.

Since the number of results is small and since according to the principles of probability it is seldom likely that in a small set of measurements the values will be actually symmetrical, the median and the average will frequently differ within the limits consistent with theoretical symmetry. For facility curves of the ordinary exponential form the average is most advantageous[2] as giving a smaller probable and a smaller huge error;[3] for curves very high in the center and widely extended at the extremes the median has the advantage for the same reasons.[4] In neither case is the advantage a great one; in fact, when the ordinary law of probability is assumed, it is practically indifferent which is used.[5]

It is noteworthy that DE FOREST apparently proves that, on the supposition of a symmetrical probability-curve, the influence of the smallness of the number of results renders a symmetrical adjustment of the mean less probable than an unsymmetrical one.[6]

B. *Assymmetrical results.* When the law of frequency is not symmetrical, the vaules x_m and x_ξ can correspond only in those cases where the centre of area falls on the centroid by some peculiar formation of the curve. Such cases, if they ever actually occur, are to be treated as cases of symmetry.

In nearly all psychological and statistical measurements the curve of probability is assymmetrical. If the abscissa of maximum ordinate be determined, the values above it will be found to be much more frequent than those below it. That is, $x_m > x_i$. For all assymmetrical curves of this general form, as the assymmetry increases, x_ξ departs more rapidly than x_m from the main mass of results,[7] and consequently does not represent them so well.

The general expression for the usual cases of assymmetry has been

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[1] NEWCOMB, *A generalized theory of the combination of observations*, Am Jour. Math., 1886 VIII 343.

[2] LAPLACE, Théorie analyt des prob, 2 Suppl., § 2.

[3] MERRIMAN, Method of Least Squares, New York 1894.

[4] EDGEWORTH, *Observations and statistics*, Trans. Camb. Phil. Soc., XIV 138 (167).

[5] EDGEWORTH, *Choice of means*, Phil. Mag., 1887 (5) XXIV 268 (270).

[6] DE FOREST, *On an unsymmetrical probability curve*, Analyst, 1883 X 67 (74).

[7] DE FOREST, *On an unsymmetrical probability curve*, Analyst, 1883 X 67.

deduced by DE FOREST.[1] It includes constants determined from the squares and cubes of the errors. Any method, however, that introduces more calculation than the usual average will be at even greater disadvantage than that value.

If FECHNER's law of the estimate of differences of sensation could be relied upon in all cases, the best representative value would unquestionably be the geometric mean. If the geometric mean g be assumed as the most probable value, it is easily shown that

$$\phi(x) = \mathrm{B}e^{a\left(\log \frac{x}{g}\right)^2}$$

which with the usual assumptions becomes[2]

$$\phi(x) = \frac{h}{\sqrt{\pi}} e^{-h^2\left(\log \frac{x}{g}\right)^2}. \tag{19}$$

The general result of experience, however, goes to show that $\phi(v)$ is of a form intermediate between (11) and (19).

For cases of assymmetry the most natural representative value to take would be x_k. As this is not determinable from few results, some other value must be used. Any other value would be justifiable only from a consideration of the purpose for which it is wanted. There is no reason, as far as I can see, for taking any one of the values around the maximum rather than any other one except in so far as it comes nearer the maximum. If we are to take x_m or x_ξ and not x_k, the middle value x_m would be the better on account of its nearness to x_k.

III. *Few results and unknown* $\phi(x)$. Since it is impossible from the few results given to make any deductions concerning $\phi(x)$, it is evident that other things being equal, that value will be preferable for which the fewest assumptions need to be made.

Let it be assumed that the curve of frequency is symmetrical. Then $x_\xi = x_m$. One value is as good as the other, for both should be the same. The value of maximum probability x_k cannot be directly calculated. For any assumed form of $\phi(x)$, a comparison of the mean variation, the mean-square-error and the probable error will

[1] DE FOREST, *On an unsymmetrical probability curve*, Analyst, 1882 IX 135, 161; 1883 X 1, 67.

[2] MCALISTER, *On the law of the geometric mean*, Quart. Jour. Pure a. Appl. Math., 1880 XVII 175.

MCALISTER, *The law of the geometric mean*, Proc. Roy. Soc. Lond., 1879 XXIX 367.

show whether they stand in the proportions required by the assumption for $\phi(x)$.

Since $\phi(x)$ is in general unknown, or only roughly suspected, it becomes desirable to either occasionally or constantly compare x_ξ and x_m in order to judge of the symmetry or assymmetry of $\phi(x)$.

For a symmetrical curve $x_\xi = x_m$. If for a supposed symmetrical curve over $x_\xi = x'_m$ the values r', s' and μ' have meanings as defined on p. ? and if r, s and μ be the corresponding values for x_m, it can be expected, according to BAYES's theorem, with a probability of

$$\Phi(\gamma) = \frac{2}{\sqrt{\pi}} \int_0^\gamma e^{-t^2}\,dt$$

that

$$\frac{r'}{\mu'} = \frac{r}{\mu} \pm \gamma \sqrt{\frac{2r^2}{\mu^2}}$$

or since

$$\frac{r}{\mu} = \frac{1}{2}$$

then

$$\frac{r'}{\mu'} = \tfrac{1}{2} \pm \frac{\gamma}{2} \sqrt{\frac{2}{\mu}}.$$

Instead of x_ξ and x_m we have $M = x_m \pm \eta$ and $A = x_\xi \pm \epsilon$ where η and ϵ are determined by (12) and (13). It would not be difficult to calculate on general principles the limits of difference between M and A within which we could, with a given degree of probability, suppose that the law is symmetrical. For the present I will assume that the desired degree of probability is $50\% = \frac{1}{2}$ and that the question to be decided is the symmetry or assymmetry of a curve whose equation is given in the case of symmetry by (11).

If the curve be symmetrical around the average, the probable error R of the variations

$$x_k - A, \qquad (k = 1, 2, \ldots, n)$$

will be the limit of variation for A. If the curve be symmetrical around the median, M can be considered as representing the average of such a curve; the probable error R′ calculated from

$$x_k - M, \qquad (k = 1, 2, \ldots n)$$

will give the limit of variation for M. Let the difference between the average and the median be denoted by

$$\delta = A - M.$$

As long as the limits of R and R$'$ overlap, that is, $\frac{R+R'}{2} < A - M$, the presence of assymmetry cannot be asserted. But when $\frac{R+R'}{2} > A - M$, it can be said with a probability of 50% that the curve is assymmetrical.

If M and A indicate symmetry in a large number of cases, either of these values can be used in similar cases for the reason that both are practically the same.

If they indicate assymmetry, there are the same reasons for prefering M to A as in the case of known $\phi'(x)$ considered above.

To Profs. Gibbs, Newton and Elkin of Yale and Prof. Merriman of Lehigh, I am under very great obligations for discussion, criticism and correction. Of course, they are in no wise responsible for my deductions or conclusions, from which each one dissents at some point. Nevertheless, any value this article may have is due to their patient labor with one who is not a mathematician but who is obliged to use mathematical means to solve practical problems.

RESEARCHES ON THE MENTAL AND PHYSICAL DEVELOPMENT OF SCHOOL-CHILDREN,

BY

J. ALLEN GILBERT, PH.D.

Infant development during the ages from 3 to 6 has been treated rather extensively, but so far as systematic scientific work is concerned, very little has been done in the study of the child during the years spent in school except in the line of bodily growth in weight, height, chest-capacity and the like. In measuring mental processes almost nothing has been done. The work of BOLTON[1] on the growth of memory in school-children has brought out a valuable subject but would have been of far greater value had its statistics, which were obtained from careful work, been presented in more intelligible form. The subject of voluntary motor ability has been treated by BRYAN.[2] Hearing has been investigated by CHRISMAN.[3]

Last year I carried through a series of tests on school-children to determine their sensitiveness to differences in pitch,[4] and through this and the problems it suggested I was led to continue my investigations on a much larger scale in order to aid in that analysis of mental phenomena which is so necessary to an understanding of child-psychology. The present investigation was undertaken with the determination to carry through a regular set or series of accurate mental and physical tests upon school-children from 6 to 17 years of age.

The work has occupied most of my time during the academical year 1893–1894, the larger part of the fall term being spent in the invention and construction of apparatus used in taking the tests. In the furtherance of my work I am specially indebted to the following persons: to Dr. E. W. Scripture, who has charge of the Laboratory and was always ready to assist any endeavors at honest work; for

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[1] BOLTON, *Growth of memory in school-children*, Am. Jour. Psych., 1893 IV 919.

[2] BRYAN, *On the development of voluntary motor ability*, Am. Jour. Psych., 1893 V 123.

[3] CHRISMAN, *The hearing of children*, Ped. Sem., 1893 II 397.

[4] GILBERT, *Experiments on the musical sensitiveness of school-children*, Stud. Yale Psych. Lab., 1893 I 80.

his assistance I am truly grateful; through his suggestions I was led to take up this investigation. To Mr. V. G. Curtis, superintendent of schools of New Haven, for the permission to enter the schools with my tests and for his kindly interest. I wish also to express my gratitude for the kindness and accommodation offered by Miss Webster, Messrs. Camp, Hurd and Thomas, principals of Welch, Dwight, Winchester and High Schools respectively; to Miss Treat, assistant at Welch for the time spent on tests (1) to (3), together with all the teachers at the respective schools who were helpful during the months occupied in taking the tests; to Dr. C. B. Bliss, fellow and assistant at the Psychological Laboratory, for much of his time and many suggestions in the preparation of apparatus and prosecution of my work throughout; to Mr. Hogan, the laboratory mechanic, for his assistance in the construction of apparatus. To Professor Williams I am indebted for the idea embodied in the suggestion-test. Valuable suggestions were also received from Professor Ladd, who has throughout shown a kindly interest in my work.

Methods and apparatus.

Each child was tested in the following respects: muscle-sense, sensitiveness to color-differences, force of suggestion, voluntary motor ability, fatigue, weight, height, lung-capacity, reaction-time, discrimination-time and time-memory. Ten sets of special apparatus were constructed for each of the first three tests.

Test (1): *Muscle-sense.*

In this test each set of the apparatus consisted of ten weights varying from 82^{g} to 100^{g} in steps of two grams each. The weights were brought to the exact weight within 50^{mg}. In order to get at mere muscle-sense or sensitiveness to weight, which is the aim of this test, heat, cold and roughness, which would distract the attention from this one sensation of weight in consciousness, must necessarily be avoided. To avoid heat and cold, cartridge shells were used, giving a paper surface, which is a non-conductor. Uncapped shells were cut in two, giving a cylinder 2.3^{cm} in diameter and 3.8^{cm} long. These were filled with lead disks and brought to within 100^{mg} of the weight desired. In order to avoid sensations of roughness, they were painted with asphalt, which leaves a hard glazed surface, raising the weight to within 50^{mg}. A cylinder of the size named and filled as described makes a weight comparatively heavy and still of

sufficiently small size to be grasped easily endwise between the thumb and finger by a six-year-old child. In cutting the lead disks with which the shells were filled, the punch was so constructed as to press the disk into convex-concave shape before cutting it from the sheet of lead. These were placed with concavity to concavity and on being pounded down of course flattened out and consequently became of diameter large enough to press firmly against the side of the shell, thus avoiding any jostling sideways within, while they were also bound tightly one by the other, thus avoiding any jostling endwise. To get them of different weights sufficient cartridge-wads were used in place of lead disks. Each weight was marked by a secret sign to indicate its weight. For each set a box was then made of appropriate size in order to avoid any mixing of the sets in taking the tests.

In taking the tests the lightest one, marked by a white speck on the end and weighing 82^g was used as the standard. The child received the box of weights and was told to sort out all those which seemed to him to be of exactly the same weight as the one with the white speck on the end, lifting them endwise between thumb and finger. To avoid the effects of fatigue on the sensitiveness to weight, each child was given only two trials on each block, lifting it and the standard alternately. The successive steps between the weights being two grams, the number of blocks selected as being of the same weight when multiplied by 2 would indicate in grams the threshold for discrimination to weight for that child.

TEST (2): *Sensitiveness to color-differences.*

Just as in the preceding test we were in search of the threshold for discrimination to weight or least perceptible difference in weight, so, in this test it was the aim to find the threshold for discrimination to color or the least perceptible difference in shade of one color. This test consisted of a series of ten shades of red so closely graded that no two successive colors or shades could be distinguished except by an experienced eye. Gray would have been preferable and in fact was tried previously to red, but owing to the fact that all goods are bleached with sulphur, no matter how well scoured before dyeing, traces of red could be found running through the gray.

Ten pieces of woolen cloth of fine texture were first dyed a suitable red by a practical dyer under my supervision. After the ten pieces were removed from this coloring solution, which left them all exactly

of the same color, a very small portion of dye was added to the boiling vat, thus making the fluid slightly darker. One of the pieces was again boiled in this, making it, when removed, very slightly darker than before. To this last solution was again added a small portion of dye in which a third piece was boiled and so on, adding for each successive piece of cloth an equal portion of dye and giving thus a series of shades each differing from the others in a very slight degree. Each of this series of ten was then fixed firmly in a ring so as to exhibit the color and yet protect it from being handled. This ring was a hollow cylinder with a narrow shoulder on one end, the edge of which was beveled so as to avoid any shadows being thrown on the color. The colored cloth was stretched firmly across the end of a circular block which was driven into the ring, holding the color firmly against the shoulder. The circle in which the color was exposed was 3^{cm} in diameter. The castings were then painted a dull black so as to avoid any reflection of light which might affect the color. Each block was then marked with a secret mark according to the place it held in the series. To avoid getting them mixed in taking the tests, a small box was made for each set of ten.

In taking the tests the block containing the lightest shade, painted white on the bottom to distinguish it, was used as the standard with which to compare the rest. The child was given a box containing one set and told to pick out all those shades of red which were exactly like the one painted white on the bottom. The number of those selected as being alike, including the standard, was then recorded. The number of colors picked out would indicate the threshold for discrimination to color for that child and by averaging the individual results the discrimination for the respective ages was obtained.

Test (3): *Force of suggestion.*

The aim in this test was to measure the effect of our ideas of a thing formed by the sense of sight upon those formed by the muscle-sense, in particular to get the effect of bulk in a thing upon our judgment of what it should weigh. As a first attempt, a set of ten blocks was made, each weighing 55^{g} with an accuracy of 50^{mg}. All were 2.8^{cm} thick but varied in diameter from 2.2^{cm} to 8.2^{cm} in a geometrical series, in order to make the sensations increase in an arithmetical series according to Weber's law. On taking a number of tests with this set, asking the subject of the

experiment to arrange them in order according to their respective weights, the decision was universal that the smallest one seemed heaviest and the largest one lightest, the others ranging between those two extremes with weights inversely proportional to their size. Now, in order to measure the amount of the suggestion offered by the bulk of the blocks, all being of the same weight in reality, a series of fourteen blocks was made, each being 2.8^{cm} long and 3.5^{cm} in diameter but all of different weights, ranging from 15^{g} to 18^{g} in weight. In order to get the different weights in the same sized blocks, holes were bored out of the center of a size directly proportional to the weight desired and filled with lead. This could then be easily bored out till the exact weight for each was reached. The lead was then concealed by a cartridge-wad, which also served the purpose of a center to the block on each end by which to grasp it when lifting. The position of each block in the series of fourteen was then marked plainly on one end.

In taking the tests for measuring the amount of suggestion offered by the difference in bulk, the very large weight and the very small weight were given to the child in connection with the series of fourteen blocks, the weight of the large and small standards being unknown to the child. He was asked to pick out of the fourteen blocks the one that seemed to him to be of the same weight as the small standard and also the one of the same weight as the large standard, lifting them endwise between the thumb and finger. The small block was first lifted; then the end-one of the 14 blocks was lifted. If it was apparently lighter, the second block of the 14 was tried. If this was also lighter, the third was tried. This was continued till that block of the 14 was reached which was apparently equal to the small block. Its number was noted. Then the large block was compared successively with those of the 14 till an apparently equal one was reached. The weight of the small one was the same as that of the large one. The amount of suggestion offered by the difference in bulk, could then be measured in grams by taking the difference in weight between the two blocks chosen as being the same weight as the large and small standards respectively. One heavier than 55^{g} was always chosen for the small one and one lighter than 55^{g} was always chosen for the large one, as will be explained in the discussion of results.

The first three tests just described were taken on desks adapted to the size of the child, thus subjecting all to the same influences in lifting. The desk was of such height as to throw the fore-arm paral-

lel with the floor. These first three tests were given to the child in the order in which I have recorded them, thus throwing the color-test in between the two weight-tests, giving no chance for the fatigue of test (1), should there be any, to be carried over into test (3).

TEST (4): *Letter-memory.*

This was omitted because of the impossibility of accurate work with letters.

TEST (6): *Weight.*

The weight of the children was taken on a balance-scale weighing with an accuracy of one-quarter of a pound. Ordinary in-door clothing was worn.

TEST (7): *Height.*

The SEAVER measuring-rod, marked off in both inches and centimeters, was used. It is composed of a straight stick with a sliding arm projecting at right angles. Placing the stick perpendicular to the floor by sighting it parallel with a door-frame, the sliding arm was made to touch the head of the child and then read off in tenths of a centimeter, as marked on the stick. The height was taken with shoes.

TEST (8): *Lung-capacity.*

The Standard wet spirometer was used. This consists of a cylindrical vessel nearly filled with water, through the center of which a tin tube projects, connected at the lower end with a rubber tube through which the experimentee exhales the air. Over this tin tube, projecting in the center of the vessel of water, a tin cylinder, closed at one end, is inverted and allowed to sink in the water by opening a stop-cock at the bottom, letting out the air confined in the vessel by the water. An index finger pointing to a scale on a support on one side, marked off in cubic inches, is fastened to the movable cylinder. As air is blown into the tube the hollow cylinder rises, marking off on the scale the number of cubic inches blown into it. The weight of the tin cylinder is balanced by weights hanging on pulleys above, so that very little pressure is required to raise the cylinder by blowing.

The child was told to inhale into its lungs all the air they possibly could contain and then to exhale it through the tube into the spirometer, emptying his lungs as completely as possible. The cubic inches were reduced to cubic centimeters for the final averages of each age.

THE REACTION-BOARD.

Since mental tests on children have never been taken to any great extent, there was consequently no suitable apparatus at hand for such tests. The carrying out of my experiments necessitated the construction of what may be called the reaction-board, arranged for taking tests (5), (9), (10) and (11). This was constructed on an oak board 33 centimeters square, fig. 5. The main parts are the electro-

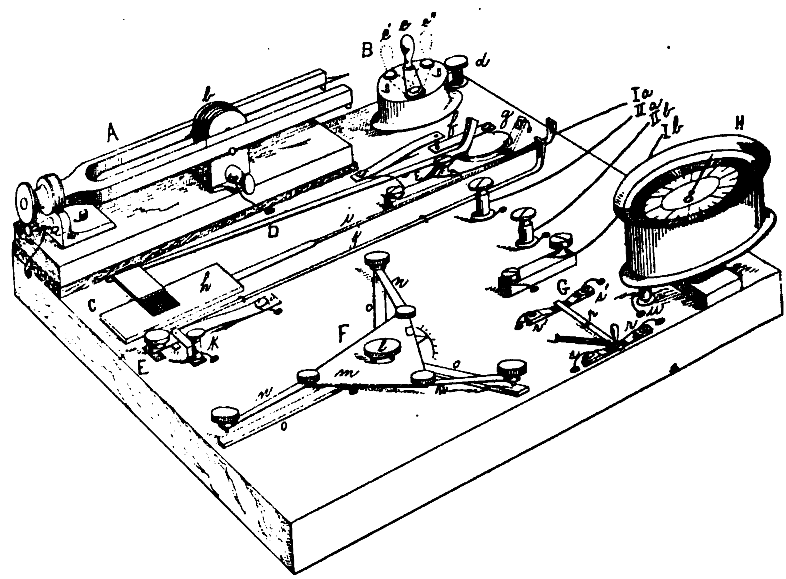

Fig. 5.

magnetic tuning-fork *A* vibrating one hundred times per second, the double-post switch *B*, the stimulating apparatus *C*, the reacting-key *E*, the tapping-apparatus *F*, the commutator *G* and the EWALD chronoscope *H*. The board is raised from the table by four short legs so as to permit insulated wires to pass beneath connecting the different parts of the apparatus through holes piercing the board. One leg *d* is an adjustable screw by which the board can be fitted to any surface upon which it may be placed. Two separate GROVE batteries had to be used, one connected with the wires I*a* I*b* and the other with II*a* II*b*. Current II*a* II*b* simply passes through the tuning-fork *A*, when the bar *p* of the commutator is left in the position in which the spring naturally holds it, viz: connecting the two posts *s* and *s'*. Thus, the tuning-fork is kept in constant motion.

The chronoscope is composed of an electro-magnet with the armature connected with a small lever, one end of which rests against a toothed wheel connected with the finger on the dial visible at *H*. Every time a current is made to pass through the electro-magnet it draws the lever, thus moving the wheel and the finger on the dial one mark. The circle on the dial of the chronoscope is divided into one hundred parts and thus, if the chronoscope is thrown into a current connecting it with the tuning fork *A*, which vibrates one hundred times a second, the finger on the dial makes one complete revolution in one second. Every time the fork vibrates the current is made at *b* by means of an adjustable wire invisible in the figure. Every time this current is made at *b* the chronoscope moves one mark and thus records the number of vibrations made by the fork, or, in other words, measures in hundredths of a second the length of time a current is allowed to pass through it and the fork. In order to test the chronoscope it was thrown into circuit with a time-marker on a smoked drum according to a method described by BLISS.[1] To verify results this test was made both before and after the taking of data. The chronoscope was found accurrate to the error of scale. Owing to the fact that the contact between *g* and the stimulating rod *D* is a "make" contact, an error of 0.005 of a second was introduced, but in as much as the chronoscope only records in hundredths of a second, this error would not influence the results.

The apparatus *F* which, for my present use, I have called the tapping-apparatus, was at first intended to be used as a habit-key. By unscrewing *l* and the screws binding the three arms *n* to the central equilateral triangular plate *m*, the plate *m* could be turned to the right thus drawing in toward the center the buttons on the ends of the bars *n*. The different radii of the circle were measured off on the scale on the face of the board as the index moved to the right or left, according as the imaginary circle, passing through the three buttons on the end of the arms *n*, was desired smaller or larger. To obtain an expression for habit, the child could be told to tap on the three buttons going in a circle to the right as fast as possible, for five or ten seconds, the number of taps being recorded by the chronoscope. After resting he could then tap the same length of time in the same circle. The increase in number of taps, or percentage of gain, in the second trial over the first, would express the rapidity of forming the muscular habit of moving the hands in a set way. This

[1] BLISS, *Researches on reaction-time and attention*, Stud. Yale Psych. Lab., 1893 I 5, fig. 3.

test was given up because it seemed impossible to get the children to keep on going in a circle should they miss one of the buttons; instead, they would almost invariably stop and try to correct their misdirected aim. Although this, however, would make an interesting test on adults, it lay outside of my problem.

Test (5): *Voluntary motor ability and fatigue.*

By throwing the arm *c* of the switch *B* to the post covered by it when in the position *c′* the current I*a* I*b* is made to pass through the wire I*a* across to the reaction-key *E*, to the binding post *g*, then through *c* in position *c′* to the tapping apparatus *F*; thence through the keys *n* and bars *o* to the chronoscope *H* and finally back to the binding post I*b*.

In measuring voluntary motor ability, the child was asked to tap as rapidly as he could, until told to stop, on the button at the end of the front key *n*. Closing the key *f* closed the current at every point except at the platinum contact between the key *n* and the bar *o*. Thus, every time the child tapped on the button of key *n* the contact was made with the bar *o* allowing the current to pass through the electro-magnet of the chronoscope, moving the finger on the dial one mark. An upright circular screen was fastened to the edge of the dial in order to hide from the child the record made. The child tapped for forty-five seconds. Shortly after he had started tapping, the circuit was closed by pressing down the key *f* in unison with one of the strokes of a metronome which was adjusted to beat seconds. As soon as the key *f* was pressed down by myself, the chronoscope commenced recording on its face each tap made by the child. At the end of five seconds I broke the current at *f* thus cutting off from the chronoscope any means of recording the taps of the child until the current was again made. The child continued tapping simply to produce fatigue. At the end of forty seconds I again made the circuit at *f* and took a record of the taps made in the last five of the 45 seconds in the same way that the first five were taken. To measure off exactly 5 seconds I counted 0, 1, 2, 3, 4, 5 in unison with the beat of the metronome, pressing down the key *f* at 0 and releasing it at 5, thus giving an interval of 5 seconds. By this means the control of the 5 seconds was in my own hands; this avoided all such errors as are sure to creep in when the child is simply told to start and stop tapping at word of command, as was done by Bryan in his researches on voluntary motor

ability.[1] In such a case it is very difficult to tell just where the counting of taps should cease, for almost invariably a child adds a stroke or two after being told to stop. By the method used here, however, the tapping is limited to exactly 5 seconds. By throwing the chronoscope into a current with the tuning-fork, vibrating one hundred times per second, I contrived a plan to measure my own accuracy in making and breaking the current at f in unison with the metronome, leaving exactly 5 seconds intervening. In 5 seconds the hand on the dial of the chronoscope should revolve 5 times. After taking and averaging fifty trials at making a 5-seconds-interval, it was found that my average variation for a single occasion in making exactly the right length of time was only 0.02 sec. Such an error would be wholly negligible since the highest rate of tapping obtained was 47 taps in 5 seconds. The number of taps made in the first 5 seconds can be taken to represent the voluntary motor ability of the child. It is impossible to tap as rapidly after 45 seconds as at first; by calculating the difference between the two rates of tapping and then dividing this difference by the number of taps made the first 5 seconds, an expression for fatigue was obtained as a per cent. If r be the number of taps for the first 5 seconds and s the number for the last 5 seconds, the degree of fatigue for 40 seconds of tapping can be expressed by

$$g=\frac{r-s}{r}.$$

To have expressed the fatigue merely by the difference between the two rates of tapping would not have expressed the truth ; for instance, one child who tapped 19 and 15 for the respective periods of 5 seconds, lost a great deal more by fatigue than another, who tapped 38 and 34 respectively; each lost 4 taps but the first lost 21 per cent., the second only 11 per cent.

Since fatigue was the principal problem I had in view in this test, the elbow was held free from the table so as to bring on fatigue the more rapidly. This also would be the most rapid way of tapping, for it consists largely of a movement of the wrist which is one of the most rapid joints for tapping.[2]

TEST (10): *Reaction-time.*

By throwing the arm c of switch B, fig. 5, to the post covered when in position c' current Ia Ib passes, when all points are connec-

[1] BRYAN, *Voluntary motor ability*, Am. Jour. Psych., 1893 V 14.

[2] BRYAN, *Voluntary motor ability*, Am. Jour. Psych., 1893 V 171.

ted, from the battery through wire I*a*, through the spring *e* to the end of the rod *D*, thence through the curved spring to *g*, thence to and through the arc *c* of switch *B*; thence to key *e* through binding post *k*; thence to *r* of the commutator through rod *p* when thrown across from *r* to *r'*; thence through the chronoscope, back again to the battery. Current II*a* II*b* is connected with the following in succession: II*a* from battery, binding post *a* on tuning-fork *A*; binding post on the coil of the fork; *s* and *s'* of commutator *G*, finally, going back to the battery through wire II*b*. The coil-spring on rod *p* keeps it continually in connection with the poles *s* and *s'*, thus keeping current II*a* II*b* always closed and the tuning-fork in constant motion, ready for use. Between *r* and *s*, *r'* and *s'* are two small pieces of hard rubber to insulate and carry the arm easily from *ss'* to *rr'*. With the arm *D* thrown back by the spring *e* against the box *h* the contact is merely broken between the end of the arm *D* and the curved spring fastened to the brass block *g*. The child is told to press down the key *E* as soon as he sees a movement of the disk fastened to the end *D*. By throwing the rod *p* of the commutator *G* upon *r* and *r'*, the current I*a* I*b* is closed at every point except where the contact is broken between the arm *D* and the curved spring on *g*. By throwing the arm *D* into the position seen in the figure this contact was made, completing the circuit and at the same time giving the stimulus for reaction. Throwing the commutator into position *rr'*, before throwing the stimulating rod *D*, served as the warning to the child that the stimulus would come, as well as changing the current used, from II*a* II*b*, which passes through the tuning-fork alone, to the current I*a* I*b* which passes through tuning-fork *A*, stimulating rod *D*, reacting key *E* and chronoscope *H*. As soon as contact was made at *g*, by throwing the stimulating rod *D*, the current, being made and passing through tuning-fork *A*, which is vibrating 100 times per second, started the chronoscope going at the rate of 100 marks or one revolution per second. As soon as the break-circuit key *E* is pressed, the current is immediately broken; thus stopping the chronoscope. This records the number of hundredths of a second which elapsed between the movement of rod *D* by myself and the pressing of the key *E* by the child. After the reaction, the key *E* is grasped and held down by a small spring *x* until the record can be read from the chronoscope and recorded. Spring *e* also throws the rod *D* back into its original position, breaking the contact at the spring on *g* as before. After a record is made on the card of the number of hundredths of a second it took the child

to react, the spring x is loosened by pushing it off the key E by the rod j and the finger of the chronoscope is moved back to 0 ready for the next test. Each child was given ten trials, the median value of which was taken for his reaction-time. In order to prevent the child anticipating the moment when the stimulating rod D is to be thrown by seeing the hand move, a screen extended back over my hand and the apparatus about the part e, f and g. This screen has been removed in the figure so as to exhibit the keys e, f and g. The face of the chronoscope H was also hidden from the child by a small semi-circular screen fastened to its edge. The EWALD chronoscope,[1] as seen in the figure, is taken from its usual stand and fastened to the board so as to place it near the commutator, thus allowing it and the commutator to be manipulated conveniently with the left hand.

TEST (9) : *Reaction with discrimination and choice.*

The apparatus for this test was the same as in simple reaction with the addition of the color apparatus h with rod i. Fastened to the rod i and concealed in the box h is a slide nearly as wide as the box but only two-thirds as long. Upon this slide are glued two pieces of colored paper, red and blue, each taking up one-half of the slide. By pushing in the rod i till it strikes at the end of the box at C, blue is exposed at the opening in the top, while red is concealed under the top of the box between the opening and the end marked C. By pulling out the rod i till it strikes the other end of the box h, red is exposed. The exposure is made when the stimulating rod D is thrown into the position seen in the figure. The child was told to react on the key E if the color, when exposed, was blue, and not to react if it was red ; this compelled him to wait and discriminate between red and blue and also to make the choice whether to react or not. First, the bar p of the commutator G is thrown from current ss' to rr', serving also as a warning to the child that the stimulating rod D will soon be thrown aside. The rod D is then thrown, whereby the chronoscope is started. Pressure on the key E immediately stops the chronoscope while the spring x also holds down the key E until the number of hundredths of a second, counted off by the chronoscope, can be recorded on the record-card. After moving the finger of the chronoscope back to 0 by a wheel invisible in the figure and releasing the spring x by pushing rod j, the apparatus is ready for the second test, the commutator p having been brought back to ss'

[1] DUMREICHER, *Zur Messung der Reactionszeit*, Inaug. Diss. Strassburg 1889.

again by the spring attached to it. That part of the rod *i* which is not concealed by the screen over the hand and keys *e*, *f* and *g*, is concealed by a wooden covering so as to prevent the child from knowing by the position of the rod what color will appear. For reasons mentioned below this test preceded that for reaction-time.

TEST (11) : *Time-memory.*

The same apparatus on the board was used here as in the preceding test, except that instead of using the stimulating rod *D* to start the chronoscope, the key *f* was used. The chronoscope, when in motion at the rate of one hundred marks a second, makes the same tone as the fork only somewhat louder and of different timbre. The tuning-fork is mounted on hair-felt so as to muffle its sound, leaving the sound of the chronoscope very easily heard. The child was told to listen how long I caused the chronoscope to sound and then after I started the sound the second time he was to stop it by pressing down the key *E* when he thought it had gone just as long as I allowed it to go the first time. After throwing the commutator from *ss'* to *rr'* to change currents and also to warn the child that the sound would soon begin, the current was made by pressing down the key *f*, which was held down till the finger on the dial of the chronoscope *H* had made two complete revolutions, whereupon it was released ; thus the sound continued for two seconds. The shade concealing the dial kept the record from being seen. Almost immediately after the current was broken, it was again made at *f*. This again started the sound which continued until the child stopped it by pressing down the key *E*. The key was held down as before by the self-catching spring *x* until the record on the chronoscope could be read and recorded. The figures entered on the card represented the error, in hundredths of a second, made by the child in trying to make the second sound just as long as the first. As it was impossible for me to make the standard exactly two seconds, this error, never more than 0.05 of a second, was added to or subtracted from the error of the child according to its direction. Each child was given ten trials from which the median value was calculated for his general result.

GENERAL METHODS.

Tests on muscle-sense, color-sensitiveness and force of suggestion were not taken simultaneously with the other tests. In taking the tests on voluntary motor ability, fatigue, weight, height, lung-capacity, discrimination-time, reaction-time and time-memory the

following order was adopted. Three children were taken from the school-room at one time into a secluded room away from interruption, noise, etc. While one was taking the tests the other two could be watching and thus, when their turns came, they understood what was expected of them, and took but little time for explanations. Furthermore, when their turns came the novelty of the board and tests was worn away, so that their whole attention could be devoted to doing the things required. The child was first weighed, his height was taken next and then his lung-capacity was measured. After this he was subjected to the tests of the reaction-board, the first being discrimination-time for red and blue. This test was given precedence in time to simple reaction because after getting accustomed to reacting every time the stimulating rod was moved, as is the case in the simple reaction, it is very much more difficult to refrain from pressing reflexly or automatically when the red is exposed in the discrimination-test than if the latter is placed first. In this test the number of errors made by pressing the key *E* when red was exposed, instead of not pressing at all, was kept account of. The red and blue were exchanged irregularly so that the child could get no idea of what color to expect. So as to put him more thoroughly on his guard, several reds were generally exposed at the start. Ten trials were given in this and in the two succeeding tests, the result of each trial being recorded in its proper place on the card as soon as taken. Reaction-time was taken next, and then the test on time-memory. Finally, after all other tests had been taken (which required from seven to ten minutes) the tests on voluntary motor-ability and fatigue were taken. During the .35 seconds interval between the first and last period of five seconds of the forty-five seconds of tapping, the next child's weight and height were taken. When the child at the board had completed the fatigue-test he was sent back to the room and immediately another came to fill his place, thus keeping three out at one time. Just before taking the reaction-board-tests on the child the day of the month and the hour of the day were noted after "date" on his card so as to enable me to calculate at some future time the effects of weather and also of fatigue produced by the day's work upon those tested late in the day, compared with those taken early in the morning. The teachers attended to having the remainder of the data filled out at the head of the card, in regard to the child, namely, age at last birthday, birth-place, birth-place of father, birth-place of mother, father's occupation. White cards were used for girls and colored cards for boys.

Results.

About one hundred children of each age were taken, the small variation from this number being shown in column N of the tables I to XI. The method explained above necessitated tests one to three inclusive being taken at a different time from the remainder. Some of the children, having taken one portion of the tests, were absent at the time the other tests were taken, thus causing the number to fall slightly below one hundred in some instances. The results for each age were averaged into one final result by taking the median value[1] according to the formula $\frac{n+1}{2}$. The justifiableness of this method will be shown later both by data and curves.

Fechner has proposed the use of the median or central value, whose position in the series of separate results arranged according to size is given by $\frac{n+1}{2}$. That is, if all the results are to be arranged in the order of their size, the median will be just in the middle. Since with finite units of measurements there will be a number of results having the same value around the middle, the value will be determined by interpolation. The importance of the use of the median lies in the fact that it involves no assumption in regard to the distribution of the separate deviations.

In order to get an expression for the homogeneity of my results the mean variation was calculated for each age. This same calculation was also made for boys and girls separately in all the tests except the first three, viz: muscle-sense, color-sensitiveness and force of suggestion. After the record-cards were completely filled out with the data desired, they were returned to the teachers of the respective rooms, who were asked to mark each name by a figure 1, 2 or 3 according to what she judged the child's general mental ability to be, marking the bright ones 1, those of average ability 2 and the dull ones with a figure 3. The aim of this was to get the relation between the general mental ability and the respective tests. The tests were taken from January 17th to April 1st, 1894.

Test (1): *Muscle-sense.*

The results for this test are shown in table I and the accompanying charts I and II, giving in graphic form the results as given in columns D, B, G and MV of the table. Sensitiveness to weight

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[1] Scripture, *On the adjustment of simple psychological measurements*, Psych. Rev., 1894 I 281.

was not so delicate as was supposed when the apparatus was made, and consequently a series of ten weights varying two grams each, from 82^g to 100^g, was insufficient to include all of the younger children. In all ages, except fourteen and fifteen, one or more children were found whose sensitiveness to weight-differences did not fall within the scope of my apparatus, viz. 18^g variance from the standard weighing 82^g. In calculating column *D* of table I, all data including those in which all ten were said to be alike in weight were used. In order to correct this error of apparatus, the per cent. of data in which all ten weights were reported as being alike was calculated for each age. To obtain a correct estimate of the discrimination to weight for each age, the columns *D* and *P* must be considered together; that is, the results for this sense give the wrong impression, unless both of the columns be considered in conjunction. The figures at the left of the chart indicate the threshold for discrimination to weight in grams. The figures found in columns *P*, *PB* and *PG* of table I represent in per cent. the number of children who picked out all ten weights as being exactly alike, distinguishing no difference whatever in their respective weights. It will be remembered that sensitiveness varies inversely as the size of the least perceptible difference. For example, the least perceptible differences for the ages 6 and 7 are 14.8^g and 13.6^g respectively; the threshold for 6 yrs. bears to that for 7 yrs. the relation $\frac{14.8}{13.6}$, but the sensitiveness for 6 yrs. is greater than that for 7 yrs., the relation being $\frac{13.6}{14.8}$. In general it is convenient to indicate the sensitiveness by the reciprocal of the least perceptible difference, thus, $\frac{1}{14.8}$, $\frac{1}{13.6}$, etc. On the chart, the higher the line the greater the least perceptible difference but the smaller the sensitiveness. Ages are marked along the axis of abscissas at the bottom of the chart.

The results show a gradual increase in ability to discriminate, from the ages of 6 to 13. At 6, the worst year of any for discrimination, the least perceptible difference was 14.8^g, with 38% of non-discriminations; at 13 years only 5.4^g with 2% of non-discriminations. After 13 there was a gradual falling off of 6.8^g, none failing to discriminate, and then another gain till at 17 it was 5.8^g with 1% of non-discriminations. Boys and girls, considered together, gradually increase in ability, but when they are considered separately, marked differences of sex appear. At 6 there is the large difference of 3.8^g in discriminative ability in favor of the boys. At 7 they have the same ability. From this on, they gain with equal pace to the year 13 with the exception of the abrupt falling off for boys at 11. From 13 to 17

the difference in ability again becomes manifest in favor of boys. In general it may be said that the superiority of boys in sensitiveness to differences in weight increases with age, irregularities being noticeable, however, from 6 to 7 and from 12 to 14.

It is interesting in this connection to notice the relation between the general curve, chart I, and the curve of mean variation for the same test, chart II. There seems to be a general agreement throughout between the main curve for discrimination and the curve for

TABLE I.

Muscle-sense.

Age.	*D*	*P*	*MV*	*B*	*PB*	*G*	*PG*	*N*	*NB*	*NG*
6	14.8	38	5.2	13.0	26	16.8	49	87	42	45
7	13.6	36	4.4	13.2	36	13.2	40	92	50	42
8	11.4	30	4.6	12.2	35	11.0	28	92	46	46
9	10.0	20	4.4	10.2	23	10.0	17	95	48	47
10	8.8	12	4.4	8.6	12	9.2	12	91	49	42
11	8.6	6	3.8	10.2	5	7.6	6	89	42	47
12	7.2	3	3.0	7.6	0	7.6	6	101	53	48
13	5.4	2	3.0	6.0	5	5.6	0	102	44	58
14	5.6	0	3.0	5.2	0	7.2	0	100	47	53
15	6.8	0	2.2	6.2	0	7.2	0	100	49	51
16	6.6	1	2.4	6.0	2	6.8	2	87	48	39
17	5.8	1	2.6	6.0	0	6.4	2	91	47	44

D, least perceptible difference in grams.
P, per cent. of data showing no discrimination.
MV, statistical mean variation.
B, least perceptible difference for boys.
PB, per cent. of boys showing no discrimination.
G, least perceptible difference for girls.
PG, per cent. of girls showing no discrimination.
N, number of children.
NB, number of boys.
NG, number of girls.

mean variation. When discriminative ability decreases, between any two successive ages, the variation decreases for the corresponding period. On the whole, however, variation decreases with advance in age. At the age of 7, where a falling off in sensitiveness is indicated in the main curve for discrimination, the mean variation is decreased. Also during the years from 12 to 14 the variation is stationary, these too, being years during which the child lost in his ability to discriminate, as shown by chart I. Apparently during these years development has been arrested and consequently those influences removed which would cause variation in the results of different children. At 15, when the discrimination was relatively poor, the mean variation was very small.

In such points the mean variation throws some light upon those years where divergences and abrupt changes occur. Marked changes in the curve for variation would indicate unusual heterogeneity in data at that point. Such results unquestionably represent changes in growth.

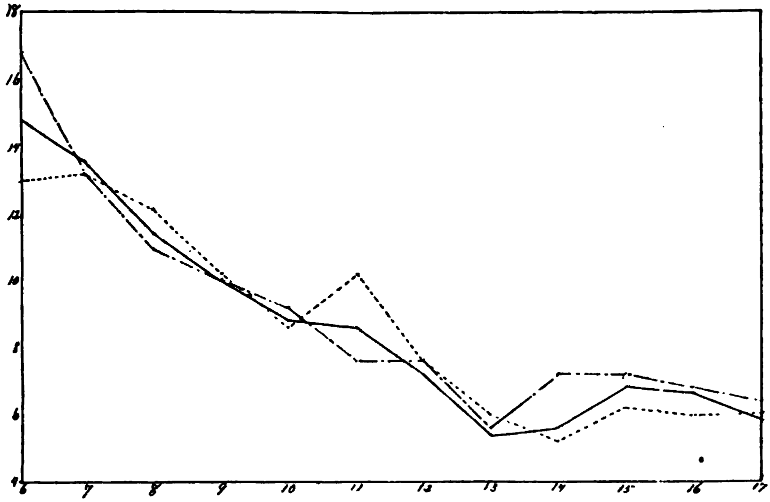

FIG. 6. CHART I.
——— Boys and girls.
.... Boys.
—·—·— Girls.

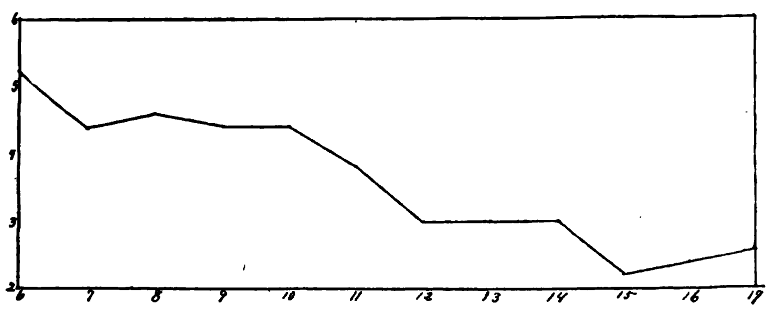

FIG. 7. CHART II.
Statistical mean variation.

TEST (2): *Sensitiveness to color-differences.*

The results of this test are recorded in table II and charts III and IV. The same general rules apply here as in the results of the previous test. The two columns D and P of table II have to be con-

sidered in conjunction, because the scale of shades was insufficient to admit of discrimination of difference in color by all children. As in the previous test the figures in column *P*, *PB* and *PG* indicate the per cent. of children of the respective ages, who said all the colors were exactly alike, discriminating no difference in shade. The numbers on the left, by which the solid line is to be interpreted, indicate the number of colors picked out as being exactly alike. The ages are indicated at the bottom of the chart on the axis of abscissas. Ability to distinguish different shades of the same color increases with age. As a rule, at 7 marked irregularities occur in all the curves which require mental action or discrimination. These irregularities will be spoken of more fully later on.

TABLE II.

Sensitiveness to color-differences.

Age.	*D*	*P*	*MV*	*B*	*PB*	*G*	*PG*	*N*	*NB*	*NG*
6	9.6	57	1.8	8.3	51	9.6	62	90	45	45
7	9.0	49	2.1	8.3	48	9.6	50	94	50	44
8	8.3	44	2.3	9.6	51	7.0	39	90	44	46
9	6.3	23	2.2	6.1	24	6.6	22	95	45	50
10	5.4	11	1.9	6.0	16	5.2	5	91	48	43
11	5.4	4	1.7	6.0	9	4.9	0	89	41	48
12	5.1	3	1.5	4.8	2	5.1	4	101	53	48
13	4.6	4	1.7	5.2	9	4.1	0	102	44	58
14	4.7	3	1.4	4.8	7	4.6	0	101	47	54
15	4.4	1	1.1	4.1	0	4.6	2	100	49	51
16	4.3	1	1.3	4.3	0	4.0	2	87	48	39
17	3.9	3	1.4	4.0	6	4.9	1	91	47	44

B, least perceptible difference in color in number of shades.
P, per cent. of data showing no discrimination.
M, statistical mean variation.
B, least perceptible difference for boys.
PB, per cent. of data of boys showing no discrimination.
G, least perceptible difference for girls.
PG, per cent of data of girls showing no discrimination.
N, number of children.
NB, number of boys.
NG, number of girls.

In this test the advantage is slightly in favor of the girls. The curves cross and re-cross so frequently, however, that no very plain statement as to comparison of sexes can be given. The boys start at 6 with the advantage of the girls, but at 17 the girls take the lead. By making a general average of all ages for all the boys and all the girls, the advantage of girls over boys is only one-tenth of the difference between the successive shades. Yet, girls have the additional

advantage in that only 18.7% of the girls failed to discriminate at all, while 22.3 % of the boys failed in so doing. This throws the final balance somewhat in favor of the girls. The curve of this sense,

FIG. 8. CHART III.
——— Boys and girls.
.... Boys.
—·—·— Girls.

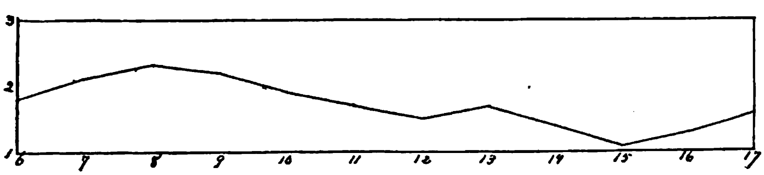

FIG. 9. CHART IV.
Statistical mean variation.

chart II, shows the most gradual *increase* in discriminative ability of any worked out and it will also be noted that the same general regularity in *decrease* of variation is shown in chart IV, with a slight divergence at 13, due probably to puberty.

TEST (3): *Force of suggestion.*

We are continually translating sensations gained from one sense into terms of another sense. In walking, or reaching for articles in the dark, we always imagine how things ought to look and then translate these ideas of sight into muscle sensations in guiding our

muscles. In reaching for the door-knob with closed eyes one always guides his hand by translating how the extended hand looks into how it should feel. The experiments of this test were taken with a view to measuring the influence of the interpretation given by one sense on the decision of another sense, the result being expressed as a function of the age.

The results obtained are recorded in table III and charts V and VI. In considering the results, columns *D* and *P*, *B* and *PB*, *G* and *PG* of table II have to be taken together as in the two preceding tests. The figures of columns *P*, *PB* and *PG* of table III indicate the per cent. of data in which the extremes of the series of fourteen blocks were picked out as of the same weight as the respective standards to be compared. The figures at the left of the chart indicate in grams the amount of error made in estimating the differences in weight between the large and small blocks. Ages are marked at the bottom of the chart. As explained under apparatus for test (3), the large and small blocks were both exactly alike in weight, but, owing to the difference in size, the child's judgment as to what the

TABLE III.

Force of suggestion.

Age.	*H*	*P*	*MV*	*B*	*PB*	*G*	*PG*	*N*	*NB*	*NG*
6	42.0	36	17.0	43.5	37	42.5	36	92	45	47
7	45.0	37	15.5	43.5	35	43.5	39	95	50	45
8	47.5	27	13.5	45.0	27	49.5	36	92	46	46
9	50.0	36	10.5	50.0	38	49.5	35	94	47	47
10	43.5	23	12.5	40.0	18	44.0	27	91	49	42
11	40.0	22	11.5	38.5	11	40.0	14	91	43	48
12	40.5	15	9.0	38.0	12	41.0	18	103	54	49
13	38.0	8	9.0	37.0	8	38.0	9	103	45	58
14	34.5	7	9.5	31.0	8	33.5	2	100	47	53
15	35.0	12	10.5	33.0	2	38.0	20	100	49	51
16	34.5	6	10.0	32.0	5	38.5	7	86	47	39
17	27.0	5	12.0	25.0	1	31.0	10	84	43	41

H, force of suggestion, in grams.
P, per cent. of data in which the force of suggestion exceeded 65 grams.
MV, statistical mean variation.
B, force of suggestion for boys, in grams.
PB. per cent. of data for boys in which the force of suggestion exceeds 65 grams.
G, force of suggestion for girls, in grams.
PG, per cent. of data for girls in which the force of suggestion exceeded 65 grams.
N, number of children.
NB, number of boys.
NG, number of girls.

blocks should weigh by muscle-sense was so influenced by the suggestion from the eye as to what their relative weight should be if judged from sight, that e. g. at 6 they thought there was a difference of 42^g between them. In addition to this, in 30% of the data more difference was made between them than could be measured by the limits of my

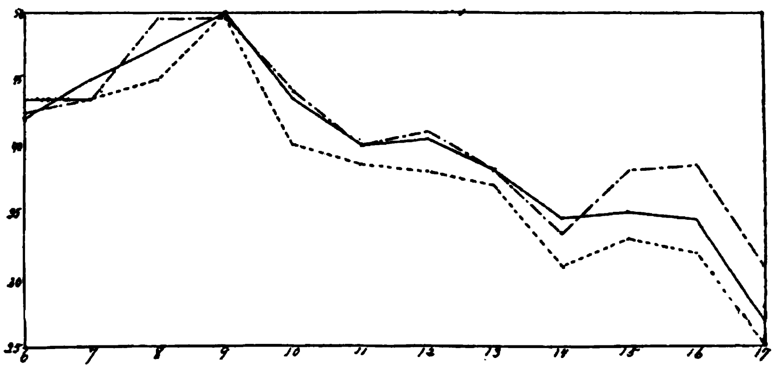

FIG. 10. CHART V.
——— Boys and girls.
.... Boys.
—.—.— Girls.

FIG. 11. CHART VI.
Statistical mean variation.

fourteen weights, viz: 65^g. At 7 they were influenced by the suggestion of sight even more than at 6. At 7 they made a difference of 45^g between the blocks while 37% said there was more difference in their respective weights than my weights would measure, viz: 65^g. The influence of the suggestion gradually increased, reaching its maximum at 9 where the average child thought there was a difference of 50^g which is almost as much as the weight of the blocks themselves, viz: 55^g. In addition to this, at 7, still 36% judged the difference larger than 65^g which was the limit of my test. From 9 to 17 this influence gradually decreased, the muscle-sense gradually

learning to correct the suggestion given by sight as to what the relative weight should be. At 17 the large difference of 27g in weight was made between the two blocks while still 5% were found whose error of judgment fell beyond the limits of measurement. As seen in columns *B*, *PB*, *G* and *PG* of table III and also as seen in curves on chart V a marked difference may be noted between boys and girls. Boys, being influenced more by the suggestion, are slightly worse at 6 than girls; at 7 both are equal; but thereafter girls are considerably worse than boys with one exception at 9, where the girls and boys may again be said to be the same since the difference is only one-half a gram. The deflection of the curve at age 14 from its general trend is again noticeable, that of the girls being most marked. It is to be presumed that the child grows worse from 6 to 9 because at 6 he has not yet learned to compare, and that as he learns gradually to judge of a thing from more aspects than one, or, in other words, learns to interpret one sense by another, the force of the suggestion given by the eye to the muscle increases until at 9 he has come to the age of experience enough to see that things are not always what they seem. Consequently at this age he begins to correct misleading influences bearing upon him. This error can never be wholly eliminated, for in all my experiments on old as well as young, I have found no one who was not subject to the illusion. The small one was universally chosen as the heavier of the two and not infrequently was it judged to be more than twice as heavy, even by adults. All those who judged that there was more difference in weight between the two blocks than 65g—the limit of my test—it will be easily seen, made the smaller one more than five times as heavy as the larger one. Reference to column *P*, table III, age 7, shows that at that age 37% gave the judgment that the large one weighed 15g or less while the small one weighed 80g or more. Since 15g and 80g were the lightest and heaviest blocks respectively in my series of fourteen, and, since so many picked out these two extremes, it is highly probable that quite a number would have made a much greater difference than 65g between the weights.

The blocks, of course, are seen before being lifted and immediately upon seeing them one judges by sight that the larger one ought to be much the heavier. However, upon lifting and receiving about the same sensation in weight from both we are immediately led to reverse our decision and judge the smaller one to be the heavier.

On the whole, variation decreases with advance in age. In the main curve, chart V, the child becomes worse in his judgment from 6 to 9. In variation, chart VI, he becomes better. In the main curve at 9 he becomes better. In variation curve he becomes worse. At ten the variation again becomes subject to the more general law, however, of decrease with age. The particular law, however, that for short periods in the development where ability increases variation increases, is substantiated by a large proportion of cases in each curve which represent a larger proportion of mental activity.

TEST (4): *Voluntary motor ability.*

In column T of table IV are recorded the number of taps the average child can make in five seconds for the respective ages. The results are given in graphic form in charts VII and VIII. The ages are marked along the axis of abscissas, and the figures to the left along the axis of ordinates represent the number of taps made in five seconds. *B* and *G* of the same table are the averages for boys and girls respectively. The average child at 6 years taps 20.8 times in five seconds. From 6 there is a gradual increase until the age 12, reaching at that age a rapidity of 29.9 taps in five seconds. At 13, however, this is lowered to 28.9 taps in five seconds. From this there is the gradual increase again, reaching the maximum at 17 with a rate of tapping amounting to 33.8 taps in five seconds. The data, when calculated for boys and girls separately, show throughout a higher rate of tapping for boys than for girls. Boys at 6 tap 21 times while girls tap only 19.7 times in the five seconds. As the increase in ability goes on, boys always excel in about the same proportion except at 14 and 17 where the difference is much more apparent.[1] Both fall off considerably from 12 to 13. At thirteen however, the boys regain their lost footing and begin to increase again as rapidly as they did before 12. The girls, however, continue to lose until 14 before beginning to gain again. From 16 to 17 they fall off once more. The divergence in this as well as the preceding curves at the period from 12 to 14 is undoubtedly due to the effects of puberty. This would contradict somewhat the statement of BURNHAM[2] who says that at puberty there is a great increase of

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[1] BRYAN, *On the development of voluntary motor ability*, Am. Jour. Psych., 1893 V 173.

[2] BURNHAM, *The study of adolescence*, Ped. Sem., 1892 I 181.

vitality and energy and also greater mental activity. The former is undoubtedly true but whether the latter is a justifiable conclusion therefrom is very doubtful. My curves throughout rather seem to justify the opinion of LANGE[1] that physical development takes up the strength and thus retards the mental development.

TABLE IV.

Voluntary motor ability.

Age.	*T*	*MV*	*B*	*MV'*	*G*	*MV''*	*N*	*NB*	*NG*
6	20.8	2.4	21.0	2.5	19.7	2.5	98	49	49
7	22.5	2.9	22.8	2.7	21.2	2.5	98	50	48
8	24.4	2.9	24.9	3.4	23.9	2.2	96	49	47
9	25.4	2.5	25.8	2.5	25.0	2.9	99	50	49
10	27.0	2.8	27.7	2.6	26.9	2.8	97	50	47
11	29.0	3.3	29.7	3.2	27.8	3.0	101	50	51
12	29.9	2.3	30.3	3.1	29.6	3.0	106	56	50
13	28.9	2.8	29.8	3.0	28.1	3.3	110	59	51
14	30.0	3.6	31.2	3.2	28.0	3.4	104	50	54
15	31.1	3.0	31.3	2.6	29.8	3.2	101	51	50
16	32.1	3.3	33.0	3.0	31.8	3.4	87	48	39
17	33.8	2.9	35.0	2.4	31.5	2.3	91	47	44

T, number of taps in five seconds.
MV, statistical mean variation.
B, number of taps for boys.
MV', statistical mean variation for boys.
G, number of taps for girls.
MV'', statistical mean variation for girls.
N, number of children.
NB, number of boys.
NG, number of girls.

However that may be, for some cause or other, the children must have labored under some disadvantage in almost all my tests at the period about 13. BRYAN's children[2] labored under some similar difficulty at about 13. The individual rate of tapping varied from 14 taps in five seconds by a couple of children 6 years old to 45 taps in five seconds by a boy 17 years of age.[3] The average variations for each age can be seen by referring to table IV and chart VIII.

[1] LANGE, *Über eine haufig vorkommende Ursache von der langsamen und mangelhaften geistigen Entwicklung der Kinder*, Zt. Psych. Phys. Sinn., 1893 VII 95.

[2] Am. Jour. Psych. 1893 V 204 charts I to V.

[3] No set amplitude of movement was given, thus allowing the child to choose that best adapted to rapidity for himself. Amplitude of movement, however, makes no special difference in rapidity; cf. BRYAN, *Voluntary motor ability*, Am. Jour. Psych., 1893 V 150, 176.

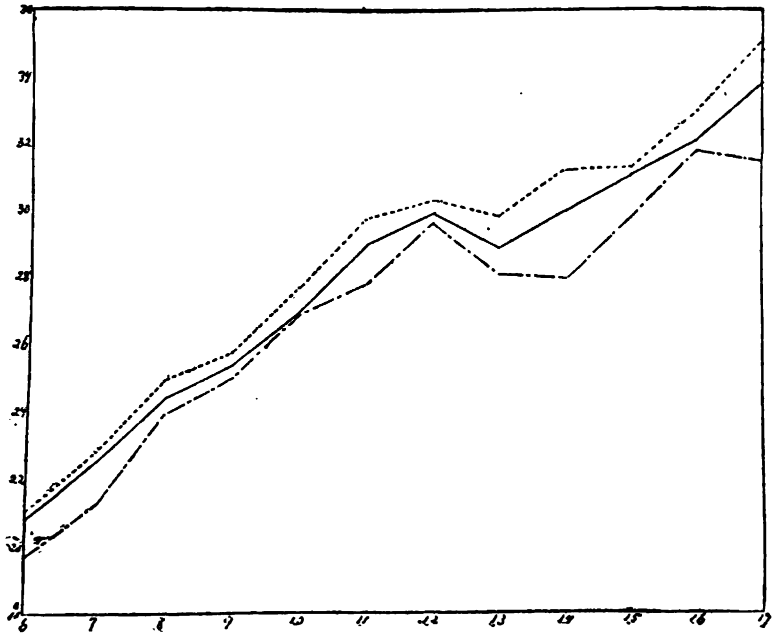

FIG. 12. CHART VII.
——— Boys and girls.
.... Boys.
—·—·— Girls.

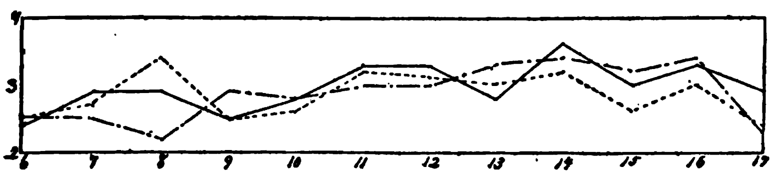

FIG. 13. CHART VIII.
——— Statistical mean variation.
.... Statistical mean variation for boys.
—·—·— Statistical mean variation for girls.

The mean variations for the total result of boys and girls combined were calculated as well as for boys and girls separately, and are found recorded in columns MV, MV' and MV'' respectively. A graphic presentation of the same is also given in chart VIII.

Test (5): *Fatigue.*

After tapping for 45 seconds fatigue entered into the results very noticeably. Column *F* of table V gives the per cent. of loss between the rapidity of tapping for the first 5 and that for the last 5 of 45 seconds. Columns *B* and *G* give the same calculation for boys and girls respectively. *MV* denotes the amount of deviation of each result from the general average while *MV'* and *MV''* indicate the same for boys and girls respectively. The same results are to be

Table V.

Fatigue.

Age	*F*	*MV*	*B*	*MV'*	*G*	*MV''*	*N*	*NB*	*NG*
6	21.4	8.1	22.8	9.4	21.3	7.0	98	49	49
7	21.0	8.9	22.5	9.7	20.2	6.7	98	50	48
8	24.0	7.3	24.7	8.3	23.3	7.1	96	49	47
9	21.0	7.1	22.5	6.7	20.7	7.8	99	50	49
10	22.0	7.5	22.7	7.8	19.0	7.1	97	50	47
11	20.0	6.2	20.3	6.5	18.0	5.5	101	50	51
12	16.0	6.3	18.0	6.0	14.0	6.7	106	56	50
13	14.5	6.4	15.8	6.7	14.7	5.8	110	59	51
14	14.0	6.5	17.8	6.2	12.0	6.1	104	50	54
15	12.7	5.8	13.8	4.9	11.5	5.7	101	51	50
16	14.7	5.2	15.3	4.6	11.7	5.6	87	48	39
17	13.8	5.3	14.5	6.3	13.5	4.3	91	47	44

F, per cent. of loss in rapidity of tapping after tapping 45 seconds.
MV, statistical mean variation.
B, per cent. of loss in rapidity for boys.
MV', statistical mean variation for boys.
G, per cent. of loss in rapidity for girls.
MV'', statistical mean variation for girls.
N, number of children.
NB, number of boys.
NG, number of girls.

found in graphic form in charts IX and X. Ages are marked at the bottom. The figures to the left of the chart indicate the per cent. of loss in rapidity of tapping between that of the first five and that of the last five seconds. The average child at 6 loses 21.4% after tapping 45 seconds. From 6 to 7 a slight gain is made, the loss by fatigue being 21% at 7. At 8, however, the effect of fatigue is much more marked, this being the age at which the child loses most rapidly; here there was a loss of 24%. After 8 the fatigue is less and less noticeable till the age of 15 where it was least marked, being only 12.7%. From 15 to 16 it again becomes more marked, a loss of 14.7% occurring at 16 with a succeeding gain again at 17, where it was 13.8%.

When these data are calculated for boys and girls separately, it becomes evident that girls tire more easily at 13 than at 12 while

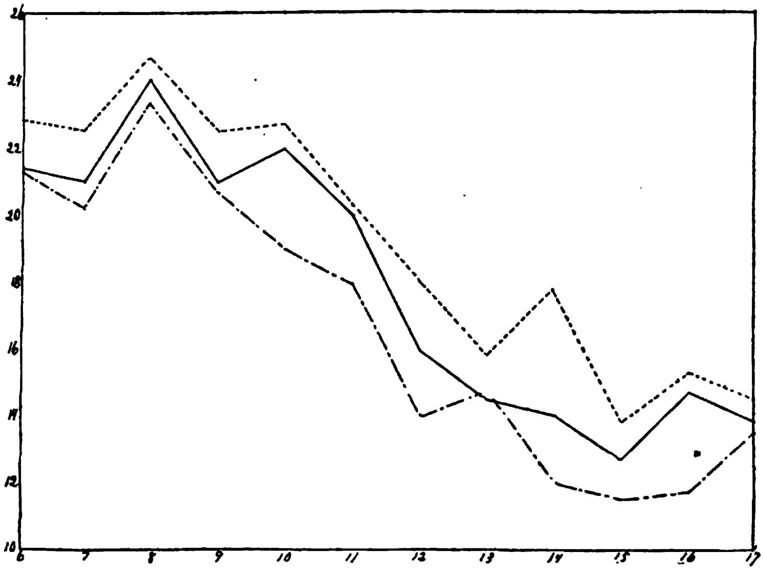

FIG. 14. CHART IX.
——— Boys and girls.
.... Boys.
—·—·— Girls.

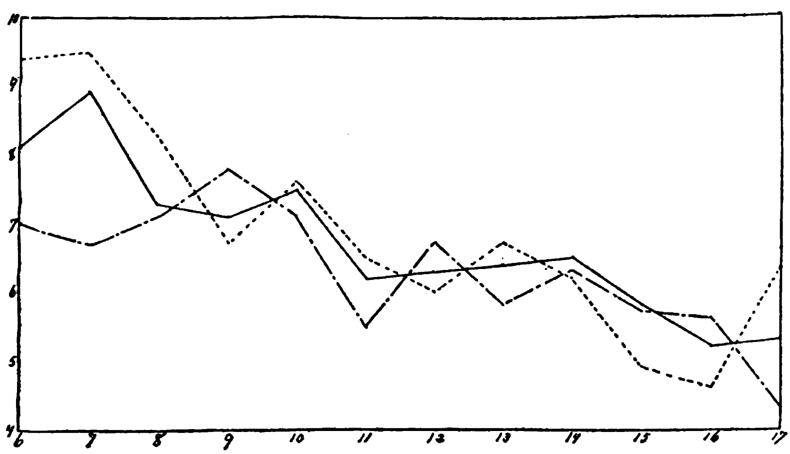

FIG. 15. CHART X.
——— Statistical mean variation.
.... Statistical mean variation for boys.
—·—·— Statistical mean variation for girls.

for boys this variance comes a year later between 13 and 14. As in almost all of the charts representing mental research, there seems

to be a marked turn in the life of the child at 7. This divergence is brought out very plainly also by the mean variations shown in chart X for ages 6 and 7.

Boys tire more *quickly* throughout in voluntary movement than girls. But the statement that boys tire more *easily* than girls could scarcely be made upon the basis of my data for it will be remembered that the rate of tapping by the boys, as shown by table IV and chart VII, was faster than that by the girls. The statement that boys tire more *easily* is unwarrantable, for, by averaging and comparing the rate of tapping for all boys and girls separately, it is found that the girls on the whole tap slower than the boys who lose but little more than the girls by fatigue, leaving the balance in favor of boys. The average boy, including all ages, taps 29.4 times in five seconds, the average girl taps 26.9 times, thus tapping 8.5% slower than boys. The average boy, including all ages, loses 18.1% by fatigue; the average girl loses 16.6%. In other words, the boys lose 1.5% more by fatigue than girls and yet boys tap 8.5% faster than girls. This leaves the balance greatly in favor of boys when voluntary motor ability and fatigue are considered together.

Test (6): *Weight.*

Column *W* of table VI indicates the weight in pounds according

TABLE VI.

Weight.

Age.	*W*	*MV*	*B*	*MV'*	*G*	*MV''*	*A*	*B*	*C*	*N*	*NB*	*NG*
6	46.0	4.6	46.8	4.4	44.3	4.3	43.3	46.8	49.0	98	50	48
7	51.0	4.8	51.2	4.7	50.4	4.4	48.3	51.0	51.5	98	50	48
8	53.0	5.9	52.5	6.0	53.0	5.1	53.3	53.5	53.3	96	49	47
9	59.5	6.2	60.0	9.9	58.8	6.8	59.0	59.0	61.5	97	49	48
10	66.5	7.7	68.4	6.9	62.7	7.4	67.2	64.5	66.8	96	50	46
11	70.0	7.8	70.8	6.7	70.0	6.0	70.0	70.0	66.5	101	51	50
12	83.5	12.3	82.3	6.7	84.5	11.5	83.0	83.3	87.8	106	56	50
13	89.5	11.6	88.0	9.4	92.0	10.6	82.2	92.3	86.0	110	59	51
14	96.0	15.4	91.7	15.8	98.0	13.3	98.5	98.8	90.3	104	50	54
15	105.0	13.7	110.0	15.4	104.0	10.5	105.5	106.0	105.0	102	51	51
16	119.8	15.4	127.0	11.9	113.0	11.7	116.0	124.5	118.3	87	48	39
17	122.0	14.9	130.0	11.3	113.7	15.1	120.0	122.8	125.0	90	46	44

W, weight in pounds.
MV, statistical mean variation.
B, weight of boys.
MV', statistical mean variation for boys.
G, weight of girls.
MV'', statistical mean variation for girls.
A, weight of bright children.
B, weight of average children.
C, weight of dull children.
N, number of children.
NB, number of boys.
NG, number of girls.

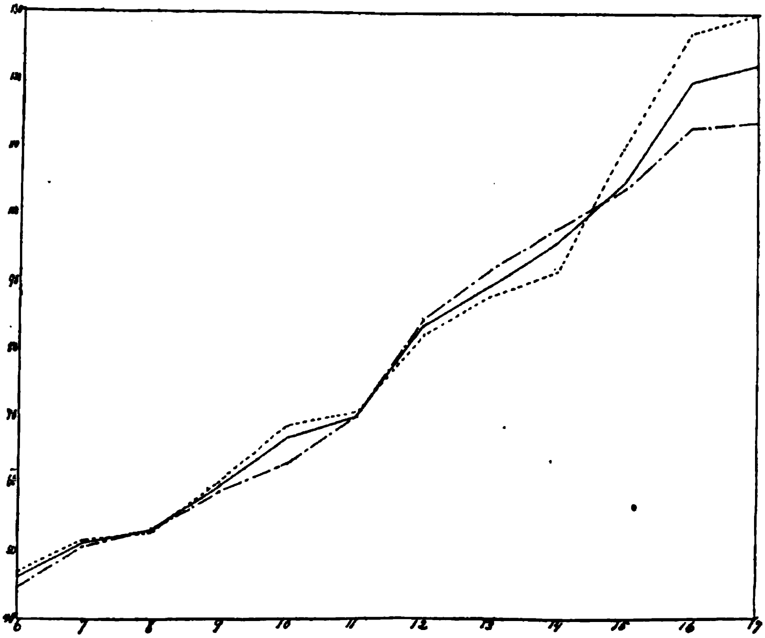

Fig. 16. Chart XI.

——— Boys and girls.
.... Boys.
—·—·— Girls.

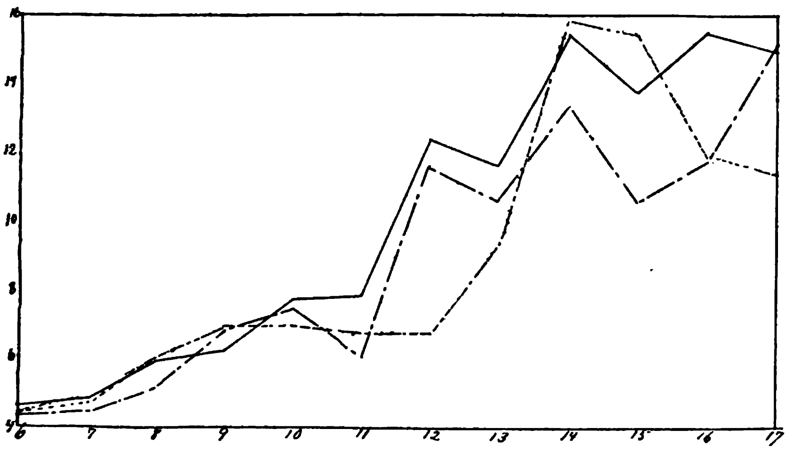

Fig. 17. Chart XII.

——— Statistical mean variations.
.... Statistical mean variation for boys.
—·—·— Statistical mean variation for girls.

to age. The individual weights were taken in quarter-pounds. The weights of boys and girls separately are to be found in columns *B* and *G*. In chart XI the ages are at the bottom and the weight in pounds to the left of the chart. The figures at the left of chart XII indicate the variation in pounds. The average weight at 6 years was 46 pounds; this in general increases with advance in age, the weight at 17 being 122 pounds. Certain differences are noticeable in the relative rapidity of growth between different ages. Boys have their most rapid growth between 14 and 16, increasing in weight 18.3 pounds between 14 and 15, and 17 pounds between 15 and 16, but between 16 and 17 the increase in weight is very slight indeed, being only 3 pounds. The most rapid growth for girls occurs between 11 and 12, being 14.5 pounds. Up to the age 12 boys and girls seem to grow in about the same proportions, boys being slightly heavier than girls. Between 11 and 12 the order is reversed, girls growing faster and becoming heavier than boys; they remain heavier until between 14 and 15. Between 14 and 15 boys again begin very rapid growth and from then on are much heavier than girls.

At the age 11, as shown in chart XI, the girls begin the period of most rapid growth. Chart XII, showing the mean variation for weight, indicates also a sudden rise in the mean variation at that time. In weight the mean variation increases with advance in years. The contrary was true in the three preceding curves where mental work was involved. It will be remembered also, that in the preceding curves, wherever there was a sudden decrease between two successive ages in ability to discriminate, there was a sudden decrease in the mean variation for the corresponding period. In these purely physiological data, however, the opposite seems to be true. The mean variation in chart XII rises at the point corresponding to the one in chart XI where the rapid growth of the girls begins. Boys in chart XI begin their rapid growth later than girls and in chart XII the sudden rise in the mean variation for the boys begins a year later. From the comparison for "bright," "average" and "dull" the same negative conclusion is to be drawn as in the following test.

Test (7): *Height.*

The height was taken in tenths of a centimeter. Column *H* of table VII is the record of height, the upper figures of each age being in inches, the lower in centimeters and tenths. The height of boys and girls separately is to be found under *B* and *G* respectively.

The average individual mean variations for all combined and for boys and girls separately are expressed in columns MV, MV' and MV'' respectively. The figures on the left of chart XIII indicate the height in centimeters. Those at the left of chart XIV indicate the variations in centimeters. The ages are marked below in both charts. Almost precisely the same laws appear here in regard to rapidity of growth for the different sexes as appeared in the figures for weight. At 6 the boys are 114.5^{cm} high, the girls 114.0^{cm}. Both boys and girls grow with about the same rapidity, the boys being the taller, until between 11 and 12; here the girls grow much more rapidly and are the taller until between 14 and 15, where the boys are taller. The girls become more nearly stationary in height after 15, while boys make exceedingly rapid progress from 14 on. At 17 the height of boys was 170.5^{cm}, of girls 168.6^{cm}. Just before puberty is the period of most rapid growth for girls while the period of most rapid growth falls later for boys, beginning at 14.

The statement has been made by PORTER that the brighter the child the taller he is.[1] Brightness and dullness, however, in his tests were decided by examination-grades, which, it is needless to say, are often very poor mental tests. In my results no such relation could be traced. My data are based upon the judgment of the teacher as to what she considered to be the general mental ability or "stand" of the child as it is sometimes called. This is really more accurate than a system of set examinations. The results as tabulated in columns A, B and C of table VII, represent the heights of the children graded according to the judgment of the teacher under whom they fell, A being bright, B those of average ability, and C those who were dull mentally. In the same results, when put in graphic form, the lines cross and re-cross too frequently to be of any value on such a point except to give the negative result as disproof of the statement referred to above.

The mean variations in chart XIV furnish one marked exception in the curve for boys to the rule applying so well to weight and even still more forcibly corroborated by the next curve of variation for lung capacity, chart XVI. Chart XIV for variations in height is not without points verifying the rule, however. Variations increase in size with advance in age. Following the curve for girls, at 10 and 11, where the period of very rapid growth begins to show

[1] PORTER, *The growth of St. Louis children*, Transactions of the Academy of Science of St. Louis, 1894 VI 335.

TABLE VII.

Height.

Age.	*H*	*MV*	*B*	*MV'*	*G*	*MV''*	*A*	*B*	*C*	*N*	*NB*	*NG*
	45.4	1.6	45.0	1.6	44.9	1.4	44.2	45.2	45.6			
6	115.4	3.9	114.5	3.9	114.0	3.6	112.3	114.8	115.8	98	50	48
	47.3	1.6	47.1	1.6	46.9	1.5	47.4	46.8	47.6			
7	120.2	4.0	119.8	4.0	119.1	3.5	120.5	119.0	121.0	98	50	48
	49.3	2.0	48.9	1.8	48.4	1.8	48.5	48.4	48.2			
8	125.2	4.9	124.2	4.4	123.0	4.4	123.3	122.8	122.3	96	49	47
	51.2	2.1	51.2	2.0	50.8	1.8	51.0	50.9	50.9			
9	130.2	5.3	130.2	5.1	129.0	4.5	129.4	129.2	129.3	97	49	48
	53.0	2.0	53.0	1.7	52.8	2.1	52.9	53.0	53.0			
10	134.6	4.9	134.6	4.3	134.0	5.2	134.4	134.6	134.5	96	50	46
	55.9	2.4	55.9	2.0	54.6	2.2	55.1	54.9	55.9			
11	142.0	6.0	142.0	5.0	138.6	5.6	140.0	139.4	142.7	101	51	50
	57.3	2.4	57.0	2.2	57.9	2.5	57.6	57.3	59.0			
12	145.5	6.1	144.8	5.6	147.1	6.3	146.2	145.5	150.0	106	56	50
	59.9	2.6	58.8	2.2	60.4	2.3	58.0	60.5	58.8			
13	151.0	6.4	149.4	5.6	153.4	5.8	147.2	153.8	149.3	110	51	59
	60.9	3.2	59.3	3.4	61.4	2.8	61.1	60.7	60.4			
14	154.7	8.1	150.5	8.7	155.9	7.1	155.2	154.2	153.3	104	50	54
	62.7	2.9	62.8	3.2	62.5	2.1	64.5	62.6	62.2			
15	159.2	7.4	159.5	8.0	158.8	5.3	163.8	159.0	158.0	102	51	51
	64.9	2.4	65.7	2.2	62.5	2.1	65.0	65.5	65.0			
16	164.8	6.0	167.0	5.4	158.8	5.2	165.1	166.3	165.2	87	48	39
	65.6	2.3	67.1	1.3	63.6	1.9	66.5	65.6	65.2			
17	166.6	5.9	170.5	3.2	161.6	4.7	169.0	166.6	165.6	91	47	44

Upper figures are inches; lower figures are centimeters.
H, height.
MV, statistical mean variation for total result.
B, height of boys.
MV', statistical mean variation for boys.
G, height of girls.
MV'', statistical mean variation for girls.
A, height of bright children.
B, height of average children.
C, height of dull children.
N, number of children.
NB, number of boys.
NG, number of girls.

itself, the variation rises rapidly. At 12, after the most rapid section of increased rate of growth is completed, the variation falls. With the exception of the one point at 14, chart XIV, which is probably due to puberty, the curve of variations gradually decreases with the corresponding decrease in rapidity of growth, shown in chart XIII. The curve for boys contradicts the rule. There is no evident

cause for the sudden fall in variation from 9 to 10 and again at 14. The curve, instead of rising with the rapid growth shown in chart XIII, falls rapidly till the age 17.

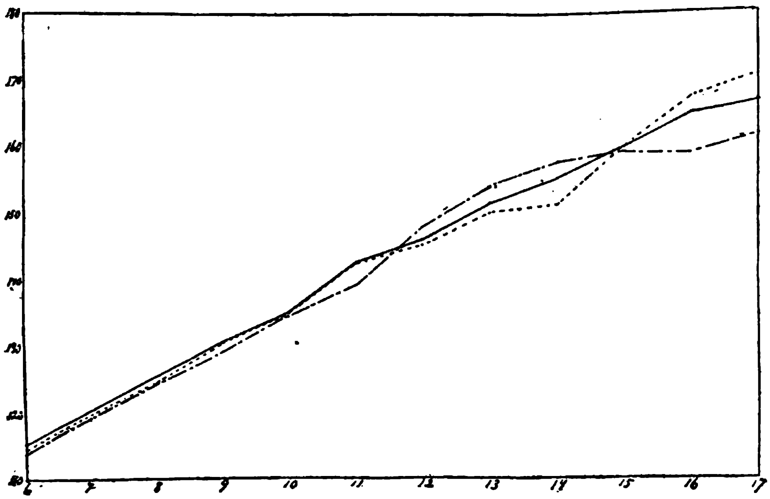

FIG. 18. CHART XIII.
——— Boys and girls.
.... Boys.
—·—·— Girls.

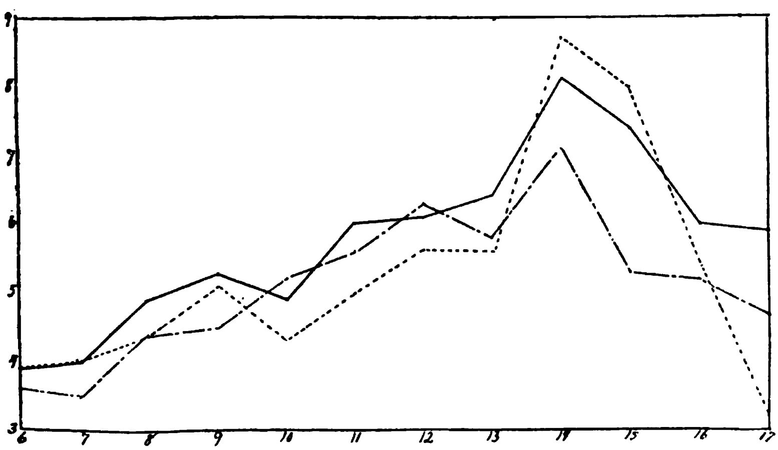

FIG. 19. CHART XIV.
——— Statistical mean variation.
.... Statistical mean variations for boys.
—·—·— Statistical mean variations for girls.

Test (8): *Lung-capacity.*

The figures in column *LC* of table VIII indicate the lung-capacity for the corresponding ages given in the first column. The upper figures of each age represent the number of cubic inches; the lower figures represent the number of cubic centimeters. These results are also placed in graphic form in chart XV, the figures at the left indicating the number of cubic centimeters, those at the bottom indicating the ages.

Table VIII.

Lung-capacity.

Age.	*LC*	*MV*	*B*	*MV'*	*G*	*MV''*	*A*	*B*	*C*	*N*	*NB*	*NG*
	52.0	9.7	56.0	9.5	50.0	9.5	52.3	51.3	49.0			
6	832	155	896	152	800	152	837	821	784	94	49	45
	63.0	11.8	66.0	11.2	54.0	11.1	67.0	60.0	67.0			
7	1008	189	1056	179	864	178	1072	960	1072	96	48	48
	72.0	12.3	73.0	11.3	66.0	10.5	72.5	68.5	67.0			
8	1152	197	1168	181	1056	168	1160	1096	1072	96	49	47
	78.0	13.8	83.0	16.3	72.5	9.8	78.0	72.0	81.0			
9	1248	221	1328	261	1160	157	1248	1152	1296	94	48	46
	87.5	14.3	91.5	15.4	82.0	14.3	90.0	84.0	80.5			
10	1400	229	1464	246	1312	229	1440	1344	1288	96	50	46
	91.0	15.9	104.0	15.1	83.0	10.4	95.0	90.0	91.0			
11	1456	254	1664	242	1328	166	1520	1440	1456	100	49	51
	109.0	16.6	113.5	14.1	104.0	14.8	108.0	110.0	113.0			
12	1744	266	1816	226	1664	237	1728	1760	1808	103	56	47
	115.0	19.4	120.0	17.5	105.0	18.8	116.0	116.0	102.0			
13	1840	292	1920	280	1680	301	1856	1856	1632	107	50	57
	117.5	22.7	125.0	23.9	105.0	17.4	119.5	118.5	97.5			
14	1880	363	2000	382	1680	278	1912	1896	1560	101	49	52
	131.3	22.3	161.0	29.8	116.0	15.5	143.5	123.0	131.0			
15	2101	356	2576	477	1856	248	2296	1968	2096	102	51	51
	149.0	39.0	187.0	30.8	115.0	16.1	137.0	166.3	128.5			
16	2384	624	2992	493	1840	258	2192	2660	2056	87	48	39
	156.0	42.0	204.0	33.4	118.5	18.5	180.0	129.5	169.0			
17	2496	672	3264	534	1896	296	2880	2072	2704	91	47	44

Upper figures are cubic inches; lower figures are cubic centimeters.
LC, lung-capacity.
MV, statistical mean variation for total result.
B, lung-capacity for boys.
MV', statistical mean variation for boys.
G, lung-capacity for girls.
MV'', statistical mean variation for girls.
A, lung-capacity of bright children.
B, lung-capacity of average children.
C, lung-capacity of dull children.
N, number of children.
NB, number of boys.
NG, number of girls.

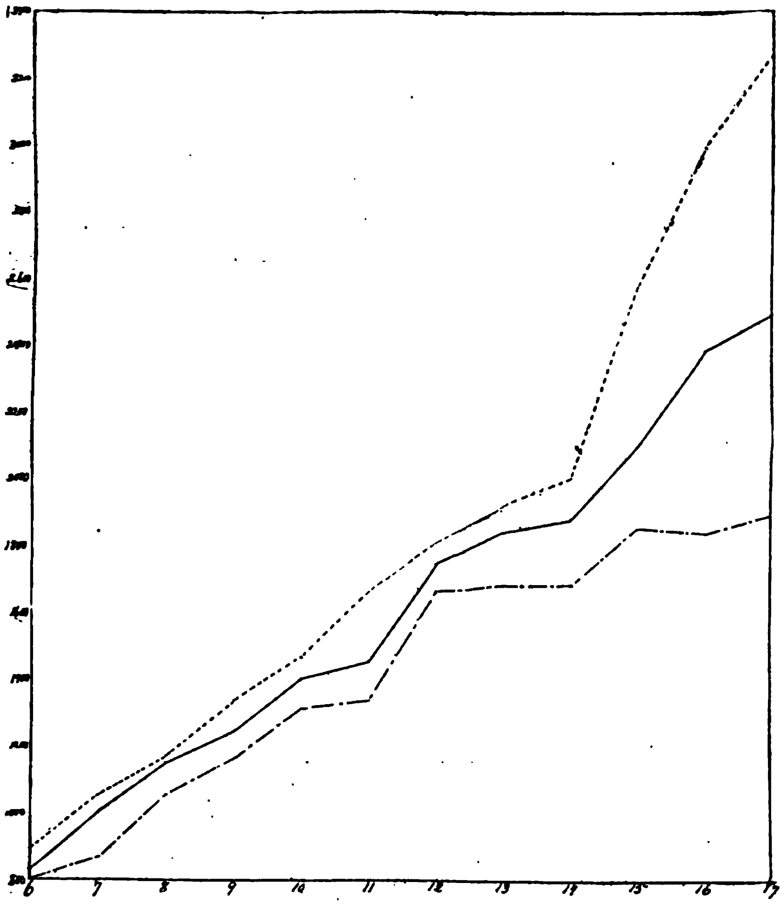

FIG. 20. CHART XV.
——— Boys and girls.
.... Boys.
—·—·— Girls.

Boys have a larger lung-capacity than girls throughout. At 6 boys have a capacity of 896ccm while the girls have only 800ccm. This difference between boys and girls increases but very slightly, in favor of boys, till the age of 12 where boys have a capacity of 1816ccm and girls 1664ccm. From this age on the development of girls is much slower. Almost no growth occurs between 12 and 14. From 14, at which age their lung-capacity is 1680ccm, a gain is made to 1856ccm at 15. From 15 to 17 almost no development of the lungs is noticeable, for at 17 they have attained only 1896ccm. The curve for boys shows a marked difference from that of girls. About the same rate of growth as that from 6 to 7 was maintained till the age of 14, at

which age their lung-capacity was 2000^{ccm}. While the girls become nearly stationary at 12, the boys do not begin their most rapid growth until 14. Between 14 and 15 they make a gain of 576^{ccm}, and they keep up this rapid growth until 17, which is as far as my experiments extended. At 17 the lungs of the average boy contained 3264^{ccm}; those of the average girl 1896^{ccm}.

The mean variations are put in graphic form in chart XVI. The numbers to the left indicate the average mean variation in cubic centimeters.

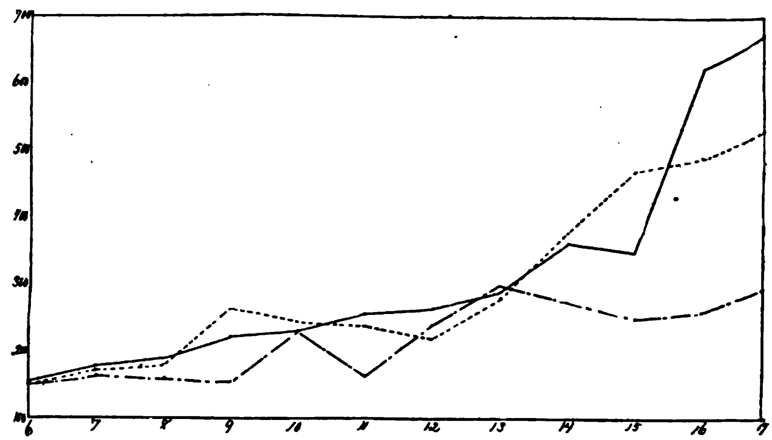

FIG. 21. CHART XVI.
——— Statistical mean variation.
.... Statistical mean variation for boys.
—·—·— Statistical mean variation for girls.

The mean variation increases with advance in years. From 6 to 13 there is a gradual rise in the variation. At this age the increase in lung-capacity almost ceases for girls and so also does the variation. Chart XV shows almost no increase in lung-capacity between the years 12 and 14. The variation for the corresponding years in chart XVI shows a fall but with a rise again at 14 where the lung-capacity begins to increase as shown by chart XV.

At 13 for boys it is just the opposite. Variations rise very rapidly as does the growth in lung-capacity, the latter appearing one year later. The coincidence between changes in growth and changes in variation are very noticeable in all the physiological curves.

The mean variation, for both boys and girls combined, as is shown in the solid line of chart XVI, increases very rapidly from 13 to 17 owing to the fact that girls after 13 grow but little more while boys undergo most rapid growth thereafter, thus of course throwing the mean variation for the total result much higher.

TEST (10): *Reaction-time.*

In taking up this section of the results we return to the mental processes in contra-distinction to the three preceding tests, which were purely physiological. In explaining apparatus and methods, tests (9) and (10) were treated in a different order from that in which they were taken, here it seems convenient and proper also to reverse the order on account of the relative complexity of the two. The simple reaction time alone will be considered first. In the following three tests the arithmetical averages were calculated as well as the median values. In order to show the difference between the results, as calculated by the two different methods I have tabulated the arithmetical averages under *Ta*, and the median values under *Tp*. The same comparison is drawn by the graphic method in the chart, the dotted line being the arithmetical averages and the solid lines the median values. In order to illustrate the method of median values I have selected from my results a card of a boy 8 years old. The point of objection to the arithmetical average is brought out somewhat more plainly in the following figures than would be the case with the average card but by choosing a somewhat extreme case, the underlying principle can be most easily seen and the difference made all the more forcible for illustration. As was explained under methods, each child was given ten trials. The results for the ten reactions of this 8-year-old boy were as follows : 24, 23, 48, 22, 21, 24, 43, 21, 22, 19. The arithmetical average of these ten is 26.7, whereas the median value, or the value half way between the fifth and sixth counting either from the largest or from the smallest as 1, is 22.5. The variations of each result from the mean value are 1.5, 0.5, 25.5, 0.5, 1.5, 1.5, 20.5, 1.5, 0.5, and 3.5 respectively, making a mean variation of 5.7. A glance at the original data will show that in order to get what we would consider the representative reaction-time of that boy, 48 and 43 ought not have as much influence given them as 21, 22 and 23 which fall nearer the average. By the median value, instead of allowing them to count in direct proportion to their size, they are rather allowed to count as one in a series of ten. However, these extreme data such as 48 and 43, are not allowed to pass unnoticed by the method of median values without exciting any influence whatever. On the contrary their influence is felt in the mean variation for the child where their effect properly belongs. In this the average of mean variations for the child is 5.7. Had it not been for the two figures 48 and 43 his mean variation would have been only 1.4 instead of the 5.7. They increase this average and

TABLE IX.
Reaction-time.

Age.	*Ta*	*Tp*	*mv*	*MV*	*B*	*MV'*	*G*	*MV"*	*A*	*B*	*C*	*N*	*NB*	*NG*
6	31.7	29.5	5.6	5.0	28.2	4.6	29.5	5.4	28.5	29.5	28.3	99	50	49
7	30.9	29.2	5.4	5.5	26.7	4.6	31.5	5.2	29.2	29.5	28.2	98	50	48
8	28.7	26.2	4.9	3.9	24.5	3.9	26.0	3.1	26.6	25.8	28.5	96	49	47
9	26.9	25.0	4.1	4.1	24.3	5.4	25.5	4.9	23.2	25.3	26.8	99	50	49
10	23.3	21.5	4.2	3.6	21.0	2.6	22.5	4.3	21.0	20.5	26.0	97	50	47
11	21.0	19.5	3.7	3.4	18.5	3.1	20.6	3.4	19.8	19.8	19.2	101	50	51
12	20.7	18.7	3.6	3.1	17.8	2.7	19.8	3.5	18.3	19.5	19.5	106	56	50
13	20.5	18.7	3.3	3.0	17.8	2.9	20.5	3.5	18.5	18.5	22.5	110	51	59
14	19.1	18.0	3.0	2.9	18.0	3.0	18.7	3.0	16.8	17.6	19.3	104	50	54
15	18.4	17.2	3.0	2.7	16.7	2.3	18.9	2.7	16.0	17.0	19.0	102	51	51
16	17.0	15.5	2.8	2.3	14.7	1.6	17.2	2.6	15.5	16.0	15.0	87	48	39
17	17.0	15.5	3.0	3.3	14.7	1.9	16.3	2.6	14.8	15.5	16.2	91	47	44

Ta, reaction-time in hundredths of a second—arithmetical averages.
Tp, reaction-time in hundredths of a second—median values.
mv, average individual mean variation.
MV, statistical mean variation.
B, reaction-time for boys in hundredths of a second.
MV', statistical mean variation for boys.
G, reaction-time for girls in hundredths of a second.
MV", statistical mean variation for girls.
A, reaction-time of bright children.
B, reaction-time of average children.
C, reaction-time of dull children.
N, number of children.
NB, number of boys.
NG, number of girls.

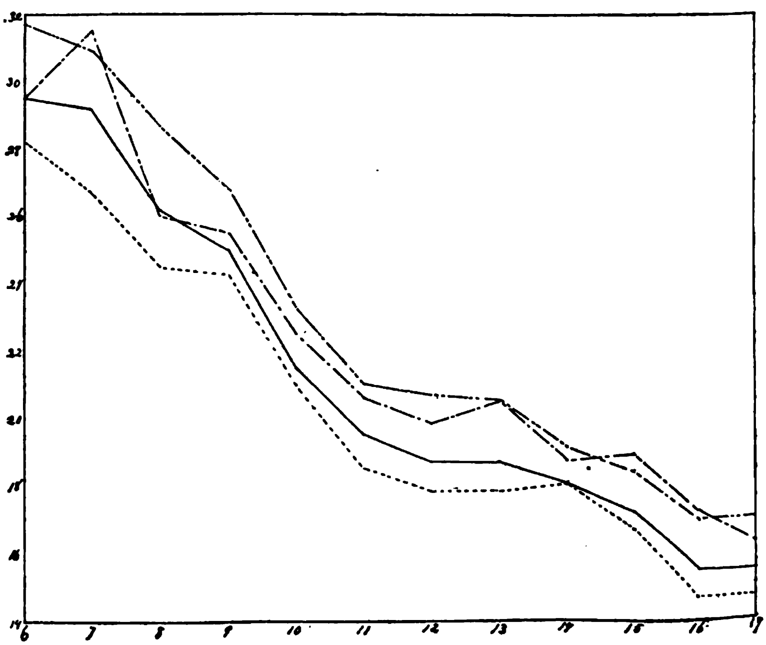

FIG 22. CHART XVII.
——————— Boys and girls.
.... Boys.
-.—.—.-.- Girls.
—-·—-·-— Boys and girls, arithmetical average.

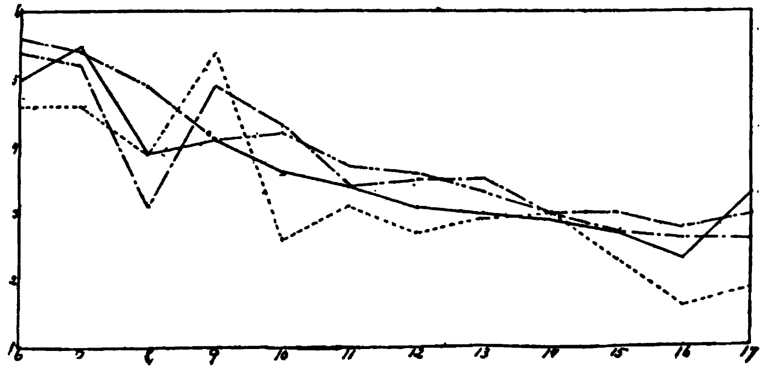

FIG. 23. CHART XVIII.

——————— Statistical mean variation.
.... Statistical mean variation for boys.
-·—·—·—· Statistical mean variation.
—·-—·-— Average individual mean variation.

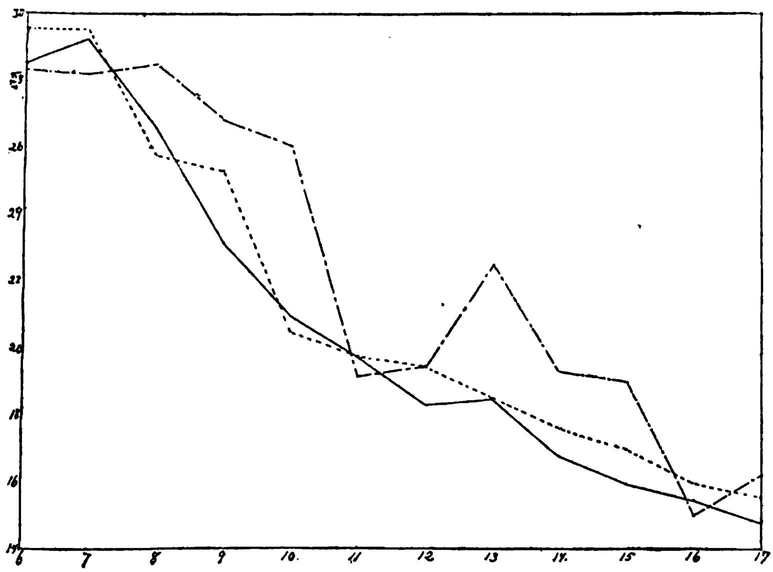

FIG. 24. CHART XIX.

——————— Bright.
.... Average.
—·—·— Dull.

justly so, for, by the mean variation we wish to indicate the irregularity of his separate results from the general average. The median was calculated for both boys and girls combined, and also for each separately. The mean for boys and girls combined according to arithmetical averages always falls half way between those for boys

and girls separately ; not so with the median. The median for boys and girls combined frequently falls below or above both that of the girls and that of boys when taken separately. Illustrations of this can be seen in charts IX, age 13 ; XII, age 7 ; XVII, age 8; XXII, age 14 ; XIII ages 6, 7, 8 and 11. This difference of the median from the average clearly shows the heterogeneity of the two classes.

It must also be noted that mean variations for boys and girls combined are much larger than for each separately, which also shows the heterogeneity of the data.

The time of simple reaction decreases with age. Boys and girls at 6, when averaged together, react in 29.5 hundredths of a second. This decreases to the age 12 where the time is 18.7 hundredths of a second. From 12 to 13 no increase is made, remaining at 18.7 for 13 also. From 13 on, there is gradual increase until 16 when the time is 15.5 hundredths of a second. At 17 no gain is made.

The results, when considered for girls and boys separately, show marked difference in sex. Girls are slower at 7 than at 6. At 6 the time required was 29.5 while at 7 they required 31.5. At 8 there was a gain to 26.0. From this on there was a gradual gain in ability and a decrease in time till 12, where the time was 19.8. At 13, however, 20.5 hundredths of a second were again required leaving the girls only one thousandth of a second better at 13 than they were at 11. After this there was an increase again till 17 where the reaction-time for girls was 16.3.

The curve for boys shows no change from the general law of increase from 6 to 7. From 12 to 14 there is a marked difference in the rapidity of increase. At 12 the time required was 17.8 ; at 13 it was the same ; at 14 there is a loss in ability, the time being 18.0. Thus the boys were worse at 14 than at 12 and but very little better than they were at 11. After 14 they again increased with almost the same rapidity as they did before 11 until 16 and 17 where 14.7 hundredths of a second were required. Both boys and girls seemed to increase less rapidly from eight to nine than at the other ages. Boys were quicker than girls throughout.

The mean variations, represented in chart XVIII, decrease with advance in years. For boys, during the period from 11 to 14 when the actual time of reaction remained almost stationary, the mean variation did the same, showing a slight difference at 12 in the same way as the curve for boys in chart XVII. There is also a marked break in the curve of variation from 8 to 9 corresponding to the slight decrease in the rate of ability at the same ages in chart XVII.

The relation between mean variation and mean reaction-time is rendered most marked by comparing the curves in charts XVII and XVIII for both boys and girls combined. At most points where there is a marked change in rate of increase in chart XVII there is also a corresponding change in the variation at that age. In this, age 13 offers the only exception. Wherever the rate of increase in ability from one age to another is less than the average rate of increase, the variation for that period becomes higher and therefore worse.

In this test also the data were separated and recalculated to find the reaction-time of those who were bright, of average mental ability and dull respectively. The results are recorded in columns *A*, *B* and *C* respectively, table IX. The difference here becomes very noticeable as can be seen by referring to the graphic representation, chart XIX. The bright children react much more quickly than the dull. Not so much difference is noticeable between those who were considered bright by the teacher and those who were judged of average ability. It is shown here that we judge of a child's mental ability by the quickness or rapidity with which they were able to act. Another fact is that all children are considered of about equal mental ability, or in other words, all grades of children react in about the same length of time just before those ages in which changes of growth manifested themselves, viz: 11 and 16. The average reaction-time of all ages for bright children was 20.7 hundredths of a second; for those of average ability it was 21.3; for dull children 22.4.

Test (9): *Reaction with discrimination and choice.*

Here, as in the other mental tests, ability increased and the length of time required decreased with advance in age. This test implies more complicated mental activity and, as would be expected, the influences which affect mental life show themselves more plainly in the curve representing such development. For some cause or other development between 6 and 7 is arrested for girls here, as well as in the test on reaction-time. Boys seem to suffer no such back-set but, starting at 53.5 hundredths of a second, continually increase from 6 till 13. From 13 to 14 they suffer a slight loss after which they gain till 17, losing slightly, however, from 15 to 16. At 17 the time required for boys was 30.5 hundredths of a second. Boys may be said to undergo only one loss, that being also of small moment. Girls suffer two marked losses, the first from 6 to 7, increasing the time required from 51 hundredths to 52.8 hundredths of a second. After 7

TABLE X.

Reaction with discrimination and choice.

Age.	*Ta*	*Tp*	*mv*	*MV*	*E*	*B*	*MV'*	*G*	*MV''*	*A*	*B*	*C*	*N*	*NB*	*NG*
6	55.8	52.5	10.2	6.0	1.1	53.5	5.3	51.0	6.5	52.3	51.0	54.5	99	50	49
7	54.1	53.0	9.4	8.1	1.1	49.0	8.8	52.8	9.4	52.0	52.7	55.5	97	49	48
8	48.8	47.8	8.5	6.5	.9	48.0	5.7	47.5	5.5	47.5	47.5	50.0	96	49	47
9	47.5	45.0	8.1	6.8	1.2	44.5	6.3	46.0	7.2	45.8	45.0	42.3	98	49	49
10	42.2	41.0	7.3	4.9	1.2	40.0	4.9	41.5	4.5	39.7	41.0	45.5	97	50	47
11	40.5	38 5	7.0	5.8	1.2	38.7	5.8	38.8	5.7	39.0	38.0	38.0	101	50	51
12	38.9	37.0	6.1	5.5	1.1	38.5	6.0	37.0	4.9	37.0	36.5	37.0	106	56	50
13	39.9	39.5	6.2	5.8	1.2	36.0	5.1	41.5	5.5	39.0	38.1	42.8	110	51	59
14	36.3	36.5	6.5	4.9	.9	36.7	4.5	35.5	5.4	33.5	36.3	36.8	104	50	54
15	34.8	33.5	5.9	4.9	.8	31.1	5.5	34.5	3.8	30.5	34.0	36.5	102	51	51
16	34.0	32.5	5.4	4.3	1.0	31.5	3.9	35.0	3.9	32.7	32.7	34.3	87	48	39
17	32.1	31.2	5.4	4.0	.7	30.5	3.5	31.5	4.4	32.5	30.0	31.2	91	47	44

Ta, discrimination-time in hundredths of a second—arithmetical averages.
Tp, discrimination-time in hundredths of a second.
mv, average individual mean variation.
MV, statistical mean variation.
E, average number of errors made by reacting to red.
B, discrimination-time for boys in hundredths of a second.
MV'. statistical mean variation for boys.
G, discrimination-time for girls in hundredths of a second.
MV'', statistical mean variation for girls.
A, discrimination-time for bright children.
B, discrimination-time for average children.
C, discrimination-time for dull children.
N, number of children.
NB, number of boys.
NG, number of girls.

they increase in ability very rapidly till the age 12, where the length of time was 37 hundredths. From 12 to 13, however, they lose just as much as they had gained during the two years preceding 12, thus requiring 41.5 hundredths of a second at 13 which is the same length of time required as at age 10. After 13 comes another very rapid gain till 17, with the exception of a small loss from 15 to 16 similar to the loss experienced by the boys at that age. At 17 the time required for girls was 31.5 hundredths of a second. Boys are better in this test than girls. The average of all the boys of all ages is 39.8 while that of the girls is 41. Not quite so much difference is seen here, however, ʼhs in the simple reaction-time where the average for boys was 20.2 while that of the girls was 22.3.

Columns *A*, *B* and *C* of table X show the length of time for bright, average and dull children respectively. In these results not quite so much difference is noticeable, which is perhaps due to the fact that they contain a somewhat smaller element of reaction-time, to which brightness and quickness are such close correlates. In this test, when all ages are considered, 40.1 is obtained as a result for bright

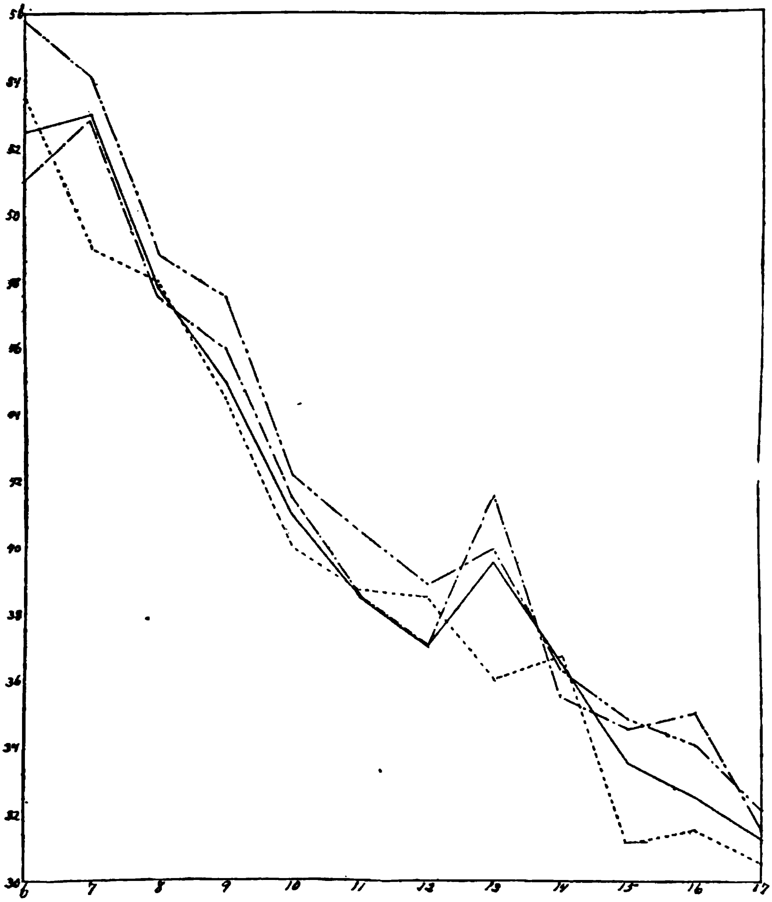

FIG. 25. CHART XX.

———— Boys and girls.
.... Boys.
-·-·-·- Girls.
—··—··— Arithmetical averages—boys and girls.

children; for average children, 40.2, and for the dull ones an average of 42.0 hundredths of a second. It is very evident from these figures that in this test the rule also applies that the brighter the child the more quickly he is able to react with discrimination and choice.

The average of the mean variations for separate children, shown by the dotted line, chart XXI, decreases gradually with age except from 12 to 14 where there is a marked increase, showing undoubtedly the effects of puberty during that period.

So far as the comparative mean variations for boys and girls are concerned, but little can be said, since the curves cross and re-cross

so frequently as to be of little value for comparisons between them and those in the main curve for development. The same general agreement exists between the main curve and the curves for variation in that there is increase in both with advance in years.

As explained under apparatus and methods the child was asked to react when blue appeared in the stimulating apparatus and not to react when red appeared. It is difficult for anybody to keep from reacting to the red, for the reaction becomes somewhat automatic and, if the attention flags, not infrequently reaction to red follows.

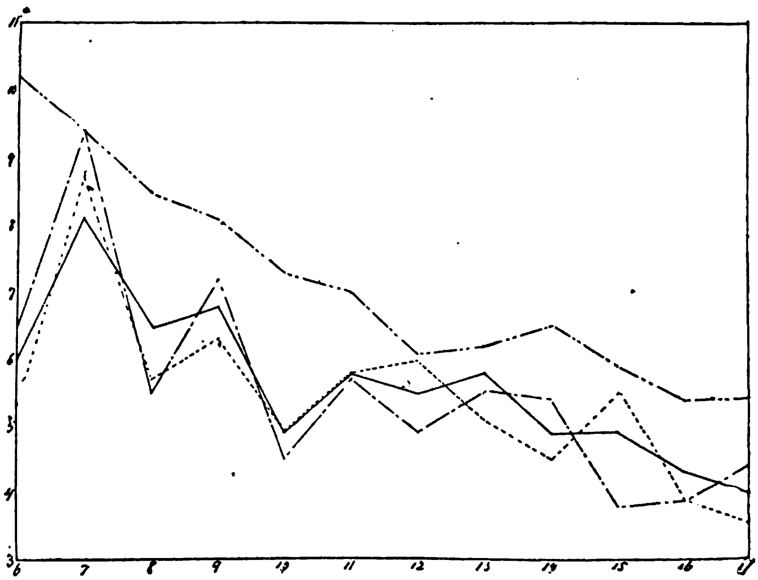

FIG. 28. CHART XXI.
——— Statistical mean variation.
.... Statistical mean variation for boys.
-·-·-·- Statistical mean variation for girls.
—··—··— Average individual mean variation.

Column *E* of table XI records the average number of reactions each child made to red out of the twenty trials given him. In order to be sure that the child discriminated between blue and red, as many reds had to be disclosed as blues, being irregularly mixed so that the numbers in column *E* would represent the number of mistakes made in 20 instead of 10, the latter of which would only represent the reactions to blue. Twenty of the 1192 children tested made the error of reacting to the red four times during the series of trials. Out of 1192 tested 420 made no errors at all by reacting to the red.

TEST (11): *Time-memory.*

As in the preceding tests, also in this one, ten trials were given each child and after taking the median value of these, the average of the mean variations for the separate results or data was calculated. Besides taking the median, the arithmetical average was taken for the sake of comparison. The arithmetical averages are to be found recorded in column *Ea* and the median values in column *Ep* of table XI. The median values for boys and girls are placed in columns *B* and *G* respectively. The averages of the mean variations for separate children are in the column headed *mv; MV* represents the mean variation for the total result, while *MV'* and *MV''* give the same for boys and girls respectively.

Columns *Ea* and *Ep* are used in constructing the two curves of chart XXV for the sake of comparing the results according to the two methods. Ages are at the bottom on the line of abscissas. The figures to the left indicate in hundredths of a second the amount of error made in making the second sound the same length as the first, which was two seconds long. The second sound was always made too short and the numbers thus indicate the amount of shortage in hundredths of a second.

The length of time, by using the arithmetical averages, is less than the median values from 6 to 10 years of age. Here, the curves, representing the result in graphic form, cross. From 10 to 15 the error is greater by arithmetical averages than by median values; from 15 to 16 the error is less; at 17 it is worse again. In calculating the results of this test the use of the method of median values is of still more importance than in the preceding tests, owing to the fact that the variations are sometimes extremely large. Frequently results were obtained in which all but one or two fell short of the correct time by 40 hundredths of a second or more while these two exceptions, owing to some disturbance or distraction either external or internal, were 20 or 25 hundredths too long. Accuracy with small mean variation demands a perfectly even flow of consciousness. During the first sound of two seconds the child simply sits and listens with no responsibility as to how long the sound shall go; all he has to do is to judge its length. When it becomes his turn to make the sound his responsibility and continuous wondering whether the sound is yet long enough make the time seem longer than it really is and consequently he stops it too soon.

The effect of suggestion in such a test is peculiar. For my tests the separate records of the child were, of course, kept secret until all

TABLE XI.

Time-memory.

Age.	*Ea*	*Ep*	*mv*	*MV*	*B*	*MV'*	*G*	*MV"*	*A*	*B*	*C*	*N*	*NB*	*NG*
6	56.7	62.0	24.6	23.4	56.5	25.1	67.0	23.2	55.5	68.5	69.5	94	52	42
7	59.6	66.5	27.9	20.2	63.5	20.6	68.5	20.4	66.8	66.5	67.3	96	49	47
8	52.7	54.3	23.6	22.8	48.5	22.3	57.0	22.3	48.5	59.3	69.0	96	49	47
9	56.2	60.0	23.0	23.5	47.5	22.4	73.5	19.3	50.0	65.0	46.5	97	49	48
10	48 9	48.5	20.2	18.1	48.5	21.8	46 5	15.8	40.3	49.5	69.5	95	49	46
11	44.2	41.0	20.8	·18.2	40.5	16.6	41.0	20.2	45.0	42.0	48.0	101	50	51
12	41.6	36.8	17.6	21.3	35.8	21.8	37.5	18.7	28.5	49.3	44.5	106	56	50
13	36.3	33.0	17.9	21.4	24.5	22.0	36.0	19.5	36.0	25.8	33.5	110	51	59
14	35.9	30.0	18.7	16.1	31.5	14.2	31.0	17.8	28.3	28.8	42.5	103	49	54
15	37.6	38.0	18.0	19.4	34.5	15.3	39.0	21.9	22.5	36.0	45.0	102	51	51
16	41.6	44.0	16 6	16.7	38.0	16.5	49.0	14.0	43.3	39.3	47.0	87	48	39
17	39.9	35.5	13.8	15.8	34.0	13.8	40.0	17.8	33.5	38.5	31.3	91	47	44

Ea, hundredths of a second short in memory of two seconds—arithmetical averages.
Ep, hundredths of a second short in memory of two seconds—median values.
mv, average individual mean variation.
MV, statistical mean variation.
B, hundredths of a second short for boys.
MV', statistical mean variation for boys.
G, hundredths of a second short for girls.
MV", statistical mean variation for girls.
A, hundredths of a second short for bright children.
B, hundredths of a second short for average children.
C, hundredths of a second short for dull children.
N, number of children.
NB, number of boys.
NG, number of girls.

had been taken. After taking the series of ten trials I again tried a number of individuals telling them each time the amount of error made. They soon learned to correct their error somewhat and not infrequently made the sound too long instead of too short.

A few of the younger children made the second sound not quite half as long as it should have been, making an error of more than 100 hundredths of a second. Only 38 out of the 1192 tested in all, judged the sound longer than it really was. It is interesting to note also that 19 out of these fell in the two ages 12 and 13, the former having 9 and the latter 10, none of the other ages having more than three each.

In time-memory the average child is worse at 7 than at 6, worse at 9 than at 8 and worse at 16 than at 14. The irregularity at the period of puberty falls later in the curves of this test than in the others. After the second decrease in ability from 8 to 9 there is a rapid increase in ability till 14. Thereafter it is reversed and there is more rapid loss than there was previous gain, leaving the child con-

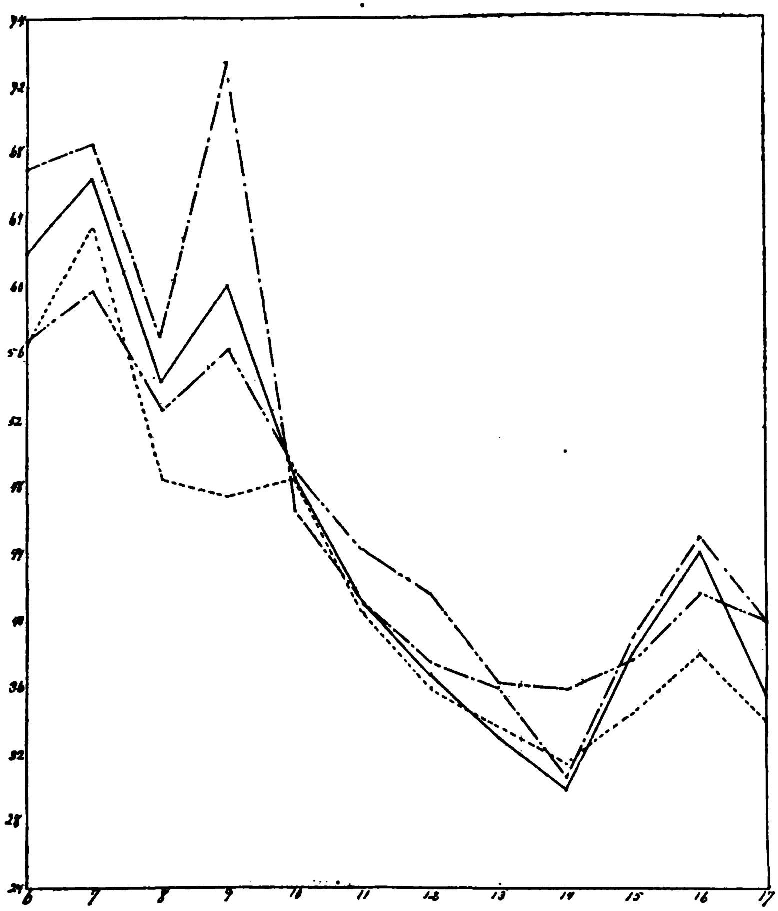

FIG. 27. CHART XXII.

———— Boys and girls.
.... Boys.
-·—·—·- Girls.
—·-—·-— Arithmetical averages.

siderably worse at 16 than he was at 12. From 16 to 17 there is another rapid increase in accuracy. At 6 the child is 62 hundredths in error; at 7 the error is 66.5; at 8 it is 54.3. After rising again at 9 to 60 there comes the rapid increase in accuracy till the best point is reached at 14 where there is only an error of 30 hundredths of a second.

The results, when divided into boys and girls, show two very distinct differences. Both boys and girls lose from 6 to 7. With the exception of a very slight change in the average increase from 8 to

10, boys grow better till the age 13. Girls from 8 to 9 undergo an enormous loss, making the sound at the latter age 73.5 hundredths of a second too short. Thus, at this age they are far worse than at any other period between 6 and 17. From 9 to 10 an exceedingly rapid gain is made and thereafter there is a continuous gain till 14, instead of stopping at 13 as the boys did. Boys and girls both lose from this point till 16 where they again begin gaining. At 13 is the best time for boys, their error being only 24.5 hundredths too short; girls reach their best point at 14 with an error of 31 hundredths of a

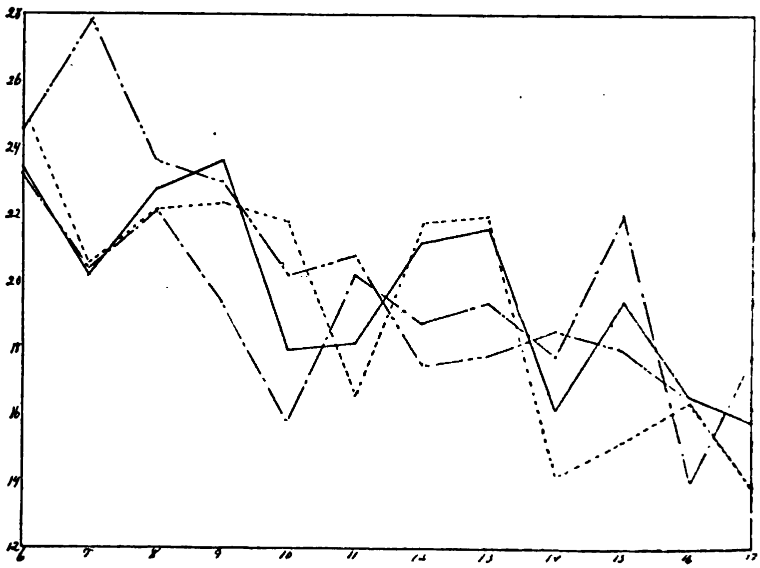

FIG. 28. CHART XXIII.
———— Statistical mean variation.
.... Statistical mean variation for boys.
–·–·–·– Statistical mean variation for girls.
—··—··— Average individual mean variation.

second. In this test also, boys greatly excel the girls, as can be seen by comparing columns *B* and *G* of table XI and also the curves of chart XXII.

In recalculating the results for comparison between bright, average and dull children the amounts of error made by them are 41.5, 47.4 and 51.1 respectively. The separate averages for each age and grade of children are recorded in columns *A*, *B* and *C* of table XI.

In general, ability increases with advance in years. This same is true of the curves of chart XXIII, representing the mean variations for time-memory. There is one thing here which is important as

corroborating the conclusion referred to under reaction-time, viz: that where a special change in relative growth between two ages occurs for the better, the mean variation changes for the worse. In chart XXIV the mean variation for both boys and girls decreases from 6 to 7, whereas the general ability for time-memory, chart XXII, increases for the corresponding period. The same can be noticed also in ages 8 to 10 and 14 to 16 where the most marked changes occurred.

General comparison of sex.

In order to get a general estimate of the mental differences of sex from the results obtained, the final averages of each age in muscle-sense, sensitiveness to color-differences, force of suggestion, reaction-time, reaction with discrimination and choice, and time-memory were thrown together into a general average for the respective ages. These general averages are recorded in table XII. The same are presented in the curves of chart XXIV. The ages are indicated at the bottom on the line of abscissas. The figures at the left can be given no definite value except to serve as relative points by which to judge the curves. The higher the figure the worse the record, just as was the case in the tests mentioned above. Voluntary motor ability and fatigue were not included in this general average both on account of their having a somewhat different nature from the rest, and, also one was the reverse of the other in that, in fatigue the lower the figure the better the record while the reverse was true for voluntary motor-ability. Could these two have been included, however, the result would have been all the more in favor of boys for it will be remembered that when these two were reduced to per cent. and considered together the balance was greatly in favor of boys. The boys became tired sooner but they also tapped much faster.

The only superiority shown for the girls is a small margin of advantage in discrimination for color-differences, referred to on page 48; the boys have the advantage very decidedly until between 7 and 8 but thereafter girls are better.

The general trend of the curves of chart XXIV is very interesting. Age 11 seems to be a neutral point where boys and girls are of about the same ability. From this age, the curves, on the whole, diverge in opposite directions more and more to the ages 6 and 17.

The general law of increase in ability is shown also very plainly by general averages taken in this way and represented in graphic form, as found in chart XXIV.

TABLE XII.

General comparison of sex.

Age.	*B + G*	*B*	*G*
6	35.1	34.1	36.4
7	36.4	34.3	36.8
8	32.9	31.6	33.3
9	33.0	30.8	35.5
10	28.4	27.7	28.5
11	25.8	25.7	25.8
12	24.5	24.1	25.0
13	23.5	21.4	24.6
14	21.9	21.5	22.1
15	22.8	21.3	24.0
16	23.2	21.4	25.4
17	20.1	19.2	22.0

The figures of the table are relative numbers deduced from the tests.

B + G, boys and girls.
B, boys.
G, girls.

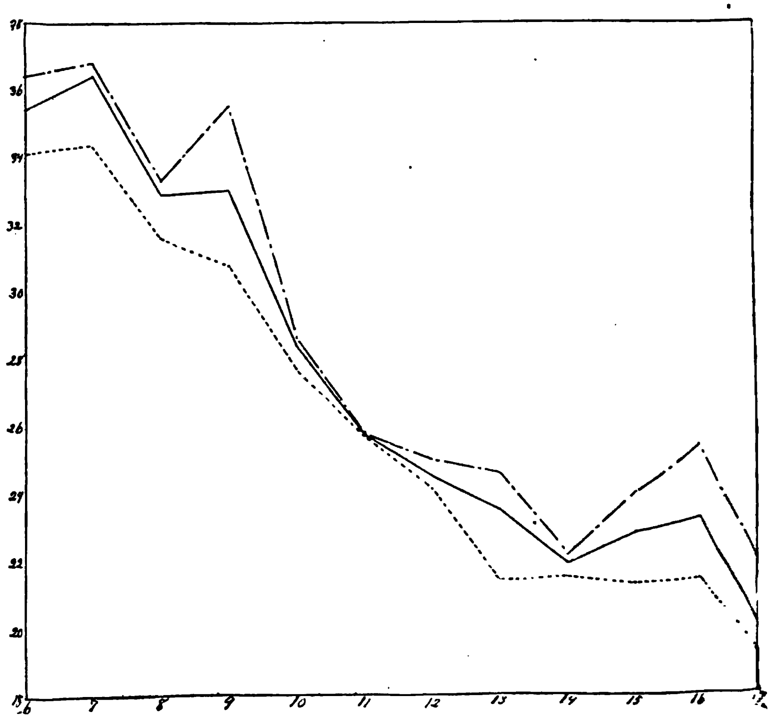

FIG. 29. CHART XXIV.

——— Boys and girls.
.... Boys.
—·—·— Girls.

INTER-RELATION OF THE RESULTS OF THE DIFFERENT TESTS.

The tests described in the preceding pages may be divided into two kinds, mental and physical. The close correlation of mind and body would naturally lead one to expect the closest sympathy between the two.

First, let us give a glance at the relations existing among the three physical curves, viz: weight, height and lung-capacity. Weight and height conform to almost exactly the same rules. In both, very slight differences exist between boys and girls until the period arrives at which boys begin to grow as men and girls as women. Boys are slightly heavier and taller than girls till between 11 and 12. Then the order is reversed. In both height and weight girls excel until between 14 and 15 where boys become heavier and taller and remain so the balance of life. The most rapid growth of girls ceases at 13 while at 14 the rapid growth for boys is just beginning. This difference between the growth of the sexes is all the more forcible when the curves for height and weight, charts XI and XIII, are compared with the one for lung-capacity, chart XV. After 12 the girls gain but very little in lung-capacity while the boys do not begin their real growth until 14. That the turning-point in life comes later for boys than for girls is verified not only by all the so-called physical curves but also by those which make the mental aspects more prominent. In all three physical curves there is the same general correspondence between the main curve for each test and its accompanying curve for mean variations. In all, the variation increases with advance in years and when separate periods of each curve are considered the rate of increase or decrease in mean variations changes wherever there is a marked change in the rate of growth. The mean variations for boys and girls in the physical curves are largest during the years from 12 to 15.

In considering the so-called mental curves there is increase in ability with advance in years with the exception of the test on the force of suggestion. In this ability decreases till 9 in the sense that the suggestion is allowed to have more force; but, thereafter there is a gradual improvement as the suggestion decreases in strength.

At first sight the curve for mean variations of sensitiveness to color-differences, chart IV, seems to offer a marked exception to the general rule, that in all mental tests the mean variations decrease with advance in years. This is not the case. The mean variation rises from 6 to 9 because during those ages a large per cent. of the data were tens, which indicates that all ten colors seemed exactly

alike to those children. Consequently, the larger the per cent. of those picking out all ten as alike, the smaller the mean variation becomes for the data at hand; but, could the 57 per cent. of non-discriminations at 6 be turned into the actual data which they could represent, the mean variation would be a great deal larger at 6 than what it is, viz: 1.8. Fifty-seven cases of non-discrimination necessitates 57 out of 100 of my data being the same: 10. This of course throws the mean variation very low.

In voluntary motor ability, chart VII, and fatigue, chart IX, more of the physical is involved, so that the relation between them and the two curves for weight and height is very marked. Voluntary motor ability from 12 to 14 is very probably affected by the very rapid growth for the corresponding period shown in charts XI and XIII. Separate relations exist for boys and girls between the curves for voluntary motor ability, fatigue and the three physical curves. The change in rapidity of growth for girls comes between 12 and 13 in weight, height and chest-capacity. The same change occurs at the same period in both voluntary motor ability and fatigue. For boys the marked change in rapidity of growth comes later than for girls, the former always changing radically at 14 as can be seen in charts XI, XIII and XV. In fatigue also the loss at puberty occurs later for boys than for girls, being at 14 instead of at 13 as it was for the girls. In voluntary motor ability the loss commences for both at the same place, viz: 12, but girls suffer loss for two successive years while boys lose only for one.

In voluntary motor ability the mean variation changes but slightly for different ages but in fatigue there is a more noticeable decrease in variation with advance in years.

The tests which are more strictly mental may be divided into two sections; the first composed of tests (1), (2) and (3) and the second of tests (9), (10) and (11).

In muscle-sense, test (1), the effect of puberty on the rusults is very marked as is shown by chart I, but in discrimination for color-differences almost no divergences whatever can be noticed at that period. In the curve for force of suggestion, boys and girls alike suffer a loss in ability from 14 to 15. The curves of mean variations in these three tests all show marked divergences from the general trend during the years from 12 to 15. By throwing the muscle-sense and force of suggestion into relation, a purely mental element is brought out in the latter. Had the weight been subjected to the muscle-sense alone the discriminative ability by that sense would

always have said they were of equal weight, for, by test (1) and chart I, it is shown that discrimination for weight increases gradually with age, but by considering the element of sight we get a measure of our error in judgment in test (3), chart V.

In tests (9), (10) and (11), where more quickness and accuracy of action is involved, the effects of puberty show themselves far more plainly than in any of those hitherto considered. Thus, it might be concluded that puberty has a greater effect on the mental than upon the physical aspects of man's nature. The difference is far more noticeable in girls than in boys as can be readily seen by referring to charts XVII, XX and XXII. This is specially noticeable in chart XXIV for the comparison of sex. The development of girls is far more seriously affected by periodic changes than that of boys as can be seen by reference to 7, 9, 14 and 16 of this chart. In discrimination time, chart XX, the loss in ability for girls comes before that of boys, the former beginning at 12 and the latter at 13. This order is reversed in time-memory, chart XXII, and comes later for both, boys beginning to lose in accurrcy at 13 and girls not until 14. This difference between discrimination-time and time-memory is brought out all the more forcibly by considering the respective curves for boys and girls combined, charts XX and XXII. In the former the loss is between 12 and 13; in the latter it occurs between 14 and 16.

The mean variation for the separate ages in tests (9), (10) and (11) furnish but little suggestion as to the effect of puberty. The averages of the individual mean variations for separate children show marked changes from 12 to 14 in discrimination and time-memory but nothing in simple reaction-time as is shown by the dash double dot lines of charts XVIII and XXVII.

In all the curves involving a time-element there is marked loss in ability from 6 to 7 as can be seen in charts XVII, XX, XXII and IX. In reaction-time and reaction with discrimination and choice, charts XVII and XX, the loss is only experienced by girls, but in the other two, time-memory and fatigue both alike suffer some set-back from 6 to 7. The same thing is noticeable to a less degree in muscle-sense and color discrimination, charts I and III, the boys alone suffering loss in color-sensitiveness. The general fact, however, is brought out very forcibly in comparative curves of sex, chart XXIV.

An interesting fact is brought out by throwing graded reaction and the same with discrimination and choice into relation with each other.

Between 11 and 12, just before puberty, in both curves the bright and dull children act with about the same rapidity, though both before and after that age the dull ones are much slower than the bright ones. It is evident from a glance at the two charts XIX and XXII that the child is judged dull at 13 because he is unable to act as quickly. Considering these charts in conjunction with the comparative curves of sex, chart XXIV, the conclusion is suggested that all children on an average are of about equal ability at age 11.

Relation of individual tests to general mental ability.

The data for the tests on weight, height, lung-capacity, discrimination-time, reaction-time and time-memory were recalculated to get the relation between the separate tests and general mental ability as estimated by the teacher. The curves for height and reaction time were inserted in part II under tests (7) and (10) respectively. The curves for height, worked out in relation to "stand" are found in chart XV and those for reaction time in chart XIX. The remainder of the curves belonging to this class have been given a separate section on account of their largely negative value. It will be remembered that the curves for height were of negative value in that no marked relation could be traced between them and the mental ability of the pupils upon whose measurement they were plotted. The same is true of weight; also of chest capacity with the exception of the years 13 and 14 where the dull pupils have a much smaller lung-capacity. The curves for reaction-time gave the most positive results showing that the brighter the child the more quickly he is able to act. In discrimination, the same relation is noticeable but to a less degree.

The bright children and those of average ability, as judged by the teachers, are about equal in the length of time required to discriminate but the dull ones require a somewhat longer time at all ages with the exception of 9, 11 and 13. The difference here between the classes is not so great owing probably to the fact that it involves a smaller element of reaction-time which shows the most marked difference. In turning to the last one, viz: time-memory, it may be said in general that the brighter the child the more accurate his sense of time. It is impossible to say that this is the case with all ages, because the curves, as can be seen, cross and re-cross so frequently as to be of but little value further than to indicate the relation of the three grades in a very general way.

COMPARISONS WITH THE RESULTS OF OTHER INVESTIGATORS.

Owing to the limited amount of investigation on the mental development of school-children but little can be said as to how my mental tests agree with other investigations. For the purely physical tests, weight, height and lung-capacity, more material presents itself. For purposes of comparison with the first two—weight and height—I have chosen the results of BOWDITCH[1] of Boston and of PECKHAM[2] of Milwaukee. The weights of the children in both these investigations as well as in mine were taken in ordinary clothes. My original calculations were made by the method of median values and the curves previously given are plotted upon this method. Since the results of both BOWDITCH and PECKHAM were obtained by arithmetical averages, for sake of comparison of the two methods, and also for comparison of localities, I recalculated my results by arithmetical

TABLE XIII.

Comparative weights of Boston, Milwaukee and New Haven school-children.

Age.	*B*	*M*	*NHa*	*NHp*	*B*	*M*	*NHa*	*NHp*
	Boys.				*Girls.*			
6	45.17	44.81	47.86	46.8	43.23	43.12	45.92	44.3
7	49.07	49.10	52.08	51.2	47.46	46 97	49.65	50.4
8	53.92	53.81	56.49	52.5	52.04	50.87	53.32	53.0
9	59.23	59.46	61.69	60.0	57.07	56.44	59.27	58.8
10	65.30	65.35	68 31	68.4	62.35	62.45	66.09	62.7
11	70.18	70.92	75.57	70.8	68.84	68.84	72.35	70.0
12	76.92	76.08	83.25	82.3	78.31	77.82	86.09	84.5
13	84.84	84.89	90.95	88.0	88.65	87.96	92.97	92.0
14	94.91	95.76	97.45	91.7	98.43	97.64	97.15	98.0
15	107.10	109.05	109.95	110.0	106.08	105.87	103.06	104.0
16	121.01	122.06	126.03	127.0	112.03	110.58	111.41	113.0
17	127.49	130.35	126.70	130.0	115.53	113.32	118.45	113.7

B, weight of Boston school-children in pounds.
M, weight of Milwaukee school-children in pounds.
NHa, weight of New Haven school-children in pounds—arithmetical averages.
NHp, weight of New Haven school-children in pounds—median values.

[1] BOWDITCH, *Growth of children*, VIII. Ann. Rept. State Board Health Mass., 307, Boston 1877.

BOWDITCH, *Growth of children*, X. Ann. Rept. State Board Health Mass., 35, Boston 1879.

BOWDITCH, *Growth of children studied by* GALTON'S *method of percentile grades*, XXII. Ann. Rept. State Board Health Mass., 479, Boston 1891.

[2] PECKHAM, *Growth of children*, VI. Ann. Rept. State Board Health Wisconsin, 28, Milwaukee 1881.

averages. Notwithstanding the protest of PECKHAM against the median, in all mental tests, at least for getting a result for each child, it is far preferable to the arithmetical averages.

PECKHAM found that Milwaukee children were heavier and taller than Boston children. New Haven children are shown by my results to be still heavier than either of the other two. The comparative weights in pounds are to be found in table XIII and also

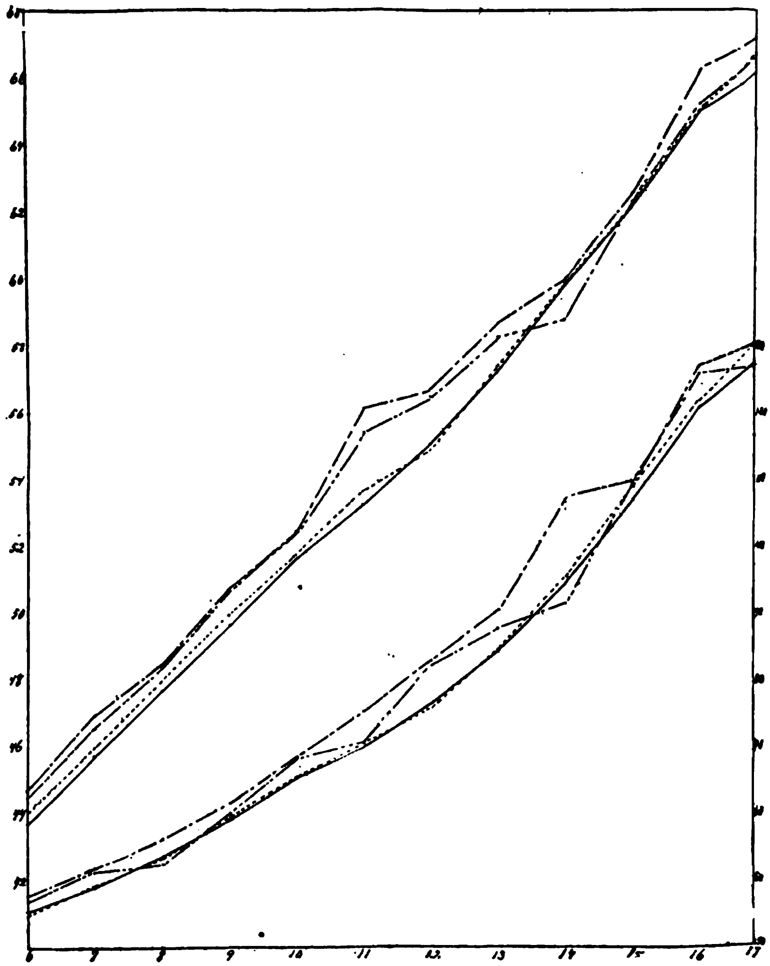

FIG. 30. CHART XXV.

Lower curves for weight.
Upper curves for height.
———— Boston boys.
.... Milwaukee boys.
-·—·-·—·- New Haven boys.
—··—··— New Haven boys—median values.

in graphic form in charts XXV and XXVI. As no relation between the arithmetical averages and median values was considered above, I have inserted here the curve of median values, as well as that of arithmetical averages. Ages are at the bottom; the figures to the left indicate height in inches; those to the right indicate weight in pounds.

The height of the children in my tests was taken with shoes; those of Bowditch and Peckham were taken without shoes. After subtracting the height of an average heel of a shoe, which is about 1 inch, the New Haven children are still taller than those of Boston and Milwaukee. The results for height are recorded in table XIV and charts XXV and XXVI.

Table XIV.

Comparative heights of Boston, Milwaukee and New Haven school-children.

Age.	*B*	*M*	*NHa*	*NHp*	*B*	*M*	*NHa*	*NHp*
	Boys.				*Girls.*			
6	43.75	44.08	45.2	45.0	43.35	43.78	45.0	44.9
7	45.74	46.09	47.4	47.1	45.52	45.93	46.9	46.9
8	47 76	48.05	49.0	48.9	47.58	47.59	48.8	48.4
9	49.69	50.00	51.3	51.2	49.37	49.81	51.1	50.8
10	51.68	51.85	53.0	53.0	51.34	51.89	53.2	52.8
11	53.33	53.76	56.7	55.9	53.42	53.80	54.1	54.6
12	55.11	54.98	57.2	57.0	55.88	56.47	58.3	57.9
13	57.21	57.47	59.2	58.8	58.16	58 68	59.6	60.4
14	59.98	59.89	60.4	59.3	69.94	60.50	60.7	61.4
15	62.30	62.34	63.1	62.8	61.10	61.59	62.2	62.5
16	65.00	65.07	66.7	65.7	61.59	62.16	62.8	62.5
17	66.16	66.60	67.1	66.1	61.92	62.91	63.8	63.6

B, height of Boston school-children in inches.

M, height of Milwaukee school-children in inches.

NHa, height of New Haven school-children in inches—arithmetical averages.

NHp, height of New Haven school-children in inches—median values.

This difference in weight and height is very likely due to the smaller proportion of foreigners included in my results. American-born children are taller and heavier than foreign-born children.[1]

So far as the relation of growth of different ages is concerned, the same general laws appear in my results as those obtained by Bowditch and Peckham.

It is interesting to notice the effect of the private-school gymnasium upon the development of lung-capacity. Table XV and chart XXVII

[1] Bowditch, *Growth of children*, VIII. Ann. Rept. State Board Health Mass., 307, Boston 1877.

represent a comparison of my results taken in public schools with the results of ANDERSON, taken in private-schools near New York. His pupils underwent a daily training in the gymnasium. The boys in his results start at 6 better than those of the same age in my results. They also develop more rapidly. No loss at puberty is traceable, however, in his results similar to that shown in mine. His results extend only to 15 years of age. At this age private-school boys had a lung-capacity of 205 cubic inches; public-school boys by my results only had a capacity of 170.3 cubic inches, and only 202.5 cubic inches at 17, which is 2.5 cubic inches less than private-school boys had at 15. In girls the difference is even more noticeable, for at 6 the girls

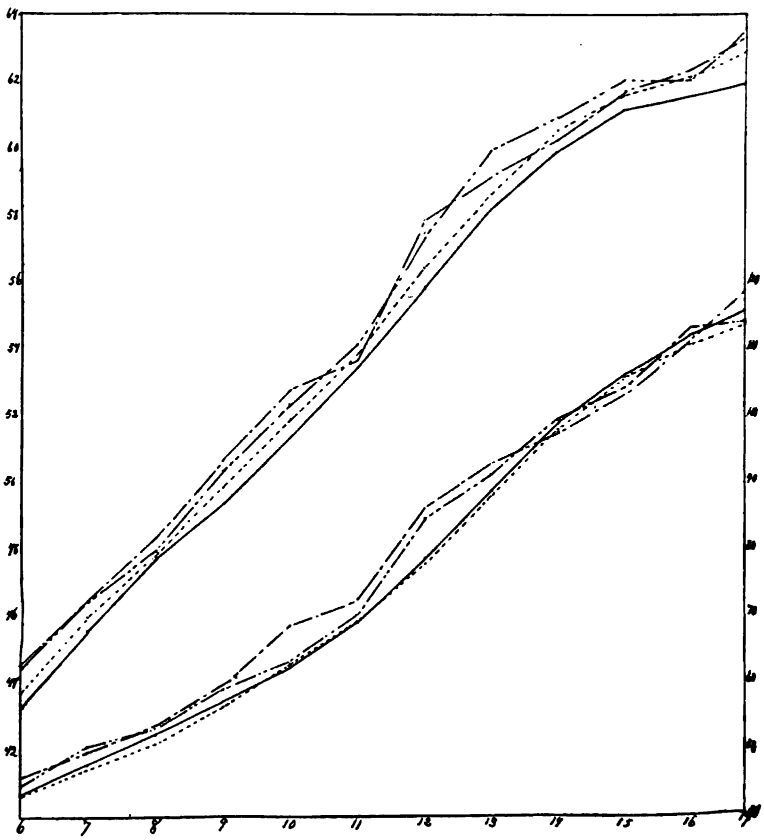

FIG. 31. CHART XXVI.

Lower curves for weight.
Upper curves for height.
———— Boston girls.
.... Milwaukee girls.
-—-—-— New Haven girls.
—--—--— New Haven girls—median values.

TABLE XV.

Comparative lung-capacities for public- and private-school children.

Age.	*P*	*NHa*	*NHp*	*P*	*NHa*	*NHp*
		Boys.			*Girls.*	
6	64	57.1	56.0	35	49.2	50.0
7	80	65.6	66.0	40	58.9	54.0
8	88	71.8	73.0	48	64.2	66.0
9	106	82.7	83.0	65	73.4	72.5
10	124	92.8	91.5	80	80.3	82.0
11	144	106.3	104.0	106	83.6	83.0
12	150	117.2	113.5	125	104.6	104.0
13	168	124.4	120.0	136	108.1	105.0
14	188	120.4	125.0	150	107.8	105.0
15	205	170.3	161.0	155	116.3	116.0
16	...	189.3	187.0	...	119.9	115.0
17	...	202.5	204.0	...	124.0	118.5

P, lung-capacity of New York private-school children.

NHa, lung-capacity of New Haven public-school children—arithmetical averages.

NHp, lung-capacity of New Haven public-school children—median values.

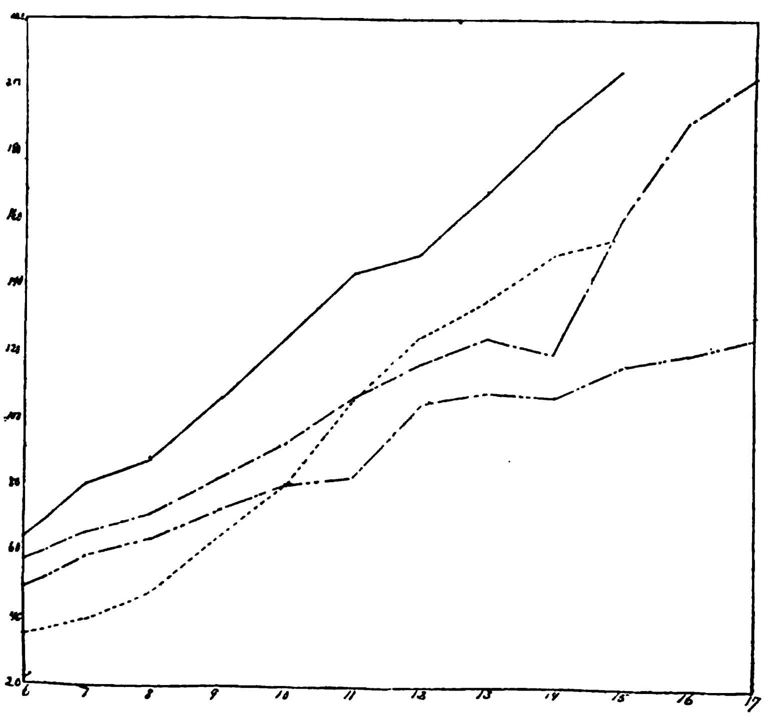

FIG. 32. CHART XXVII.

———— Private school boys.
.... Private school girls.
-·—·—·- Public school boys.
—··—··— Public school girls.

at private schools have a smaller lung-capacity but develop so rapidly as at 10 years of age to equal those of public schools. Thereafter the rapid development continues so that at 17 they have attained 155 cubic inches while my results only show 116.3 cubic inches for public-school girls; even at 17, public-school girls have only 124 cubic inches capacity, which is 31 cubic inches less than that of private-school girls at 15. The curves of chart XXVII show an enormous difference between the two classes of children. There is added, however, a note to ANDERSON's table: "It cannot be said of them that they indicate just what the averages should be." The results of this table, however, were averages of about 600 children of each age.

In chart XXVII the ages are at the bottom and the figures at the left indicate the lung-capacity in cubic inches.

REMARKS ON DR. GILBERT'S ARTICLE,

BY

E. W. SCRIPTURE.

In preparing for publication Dr. GILBERT's *Researches on the mental and physical development of school-children*, several matters seemed to me to require a further statement.

Suggestion-test. The large and the small standard are of the same weight, namely, $w=55^g$. The diameter of the small standard was $d=2.2^{cm}$, and of the large standard $D=8.2^m$. The weights of the 14 blocks were successively

$$p_1=15^g;\ p_2=20^g;\ .\ \ .\ \ .;\ p_{14}=80^g,$$

the difference between any two successive blocks being $p_i-p_j=5^g$. The diameter was $\delta=3.5^g$ for all. The result of the child's two judgments was that $w=p_k$ for the small standard and $w=p_l$ for the large standard. The amounts of difference $v_k=p_k-w$ and $v_l=p_l-w$ can be taken as measures of the effect of the different sizes of the standard and the blocks of the 14-series. As all blocks were cylinders of the same length, we can, if we neglect effects of contrast of length to diameter, express the difference in size by the areas of the ends. Thus the visual suggestions can be indicated by $\frac{1}{4}(\pi\delta^2-\pi d^2)=u_k$ and $\frac{1}{4}(\pi D^2-\pi\delta^2)=u_l$ respectively. As the differences v_k and v_l disappear when the blocks are lifted without being seen, we can put

$$v_k=f_k(u_k) \text{ and } v_l=f_l(u_l).$$

Full expressions for f_k and f_l for a given individual would give the law of suggestion for the given case. The determination of this law was not the object of the investigation, in which u_k and u_l were taken constant. For the sake of brevity the difference between the large block and the small block was taken as the amount of suggestion ; thus the constant suggestion was taken as

$$S=u_k+u_l=\tfrac{1}{4}(\pi D^2-\pi d^2).$$

The result of this suggestion is

$$H=v_k+v_l.$$

If by A we indicate the age then

$$H=f(S, A),$$

and by taking $S=$ constant, we obtain

$$H=f(A)$$

or the force of suggestion as a dependent on age.

Method of computation. The method of computing the results can be thus indicated. Let the results be

$$\begin{array}{llll} a_1, & a_2, & \ldots, & a_n \\ b_1, & b_2, & \ldots, & b_n \\ \cdot & \cdot & & \cdot \\ \cdot & \cdot & & \cdot \\ \cdot & \cdot & & \cdot \\ l_1, & l_2, & \ldots, & l_n \end{array}$$

where the letters $a, b, \ldots, l$ refer to different children and the indices $1, 2, \ldots, n$ refer to experiments on the same child.

If we express by $R_s=f_i(x)$ the fact that R is determined from the i powers of x, then for the median we have the general expression

$$M_s=f_0(x_1, x_2, \ldots, x_n)$$

and for the arithmetic mean

$$A_s=f_1(x_1, x_2, \ldots, x_n).$$

For the reasons indicated on p. 23 the mean variations are determined as f_1.

We have thus for the individual children the medians

$$\begin{array}{l} c_a=f_0(a_1, a_2, \ldots, a_n) \\ c_b=f_0(b_1, b_2, \ldots, b_n) \\ \cdot \quad \cdot \quad \cdot \\ \cdot \quad \cdot \quad \cdot \\ \cdot \quad \cdot \quad \cdot \\ c_l=f_0(l_1, l_2, \ldots, l_n) \end{array}$$

The mean variations for the individual children will be

$$\begin{array}{l} D_a=f_1([a_1-c_a], [a_2-c_a], \ldots, [a_n-c_a]) \\ D_b=f_1([b_1-c_b], [b_2-c_b], \ldots, [b_n-c_b]) \\ \cdot \quad \cdot \quad \cdot \quad \cdot \\ \cdot \quad \cdot \quad \cdot \quad \cdot \\ \cdot \quad \cdot \quad \cdot \quad \cdot \\ D_l=f_1([l_1-c_l], [l_2-c_l], \ldots, [l_n-c_l]) \end{array}$$

These mean variations can be regarded as psychological quantities expressing the accuracy of each child's judgment. The median accuracy of judgment for the particular age r will be

$$D_r = f_0(D_a, D_b, \ldots, D_l).$$

This can be called the mean personal insecurity.

These individual quantities are quite different from the statistical mean variations, which are taken in order to show the homogeneity of the children for any given age. The median for a given age r is found by

$$C_r = f_0(c_a, c_b, \ldots, c_l).$$

The mean variation of the children from the general mean will be

$$D_r = f_1([c_a - c_r], [c_b - c_r], \ldots, [c_l - c_r]).$$

Limited series. In tests (1), (2) and (3) it was discovered too late that the series of blocks and colors did not extend far enough to include extreme cases. In each table there is a column with the percentage of cases where the least perceptible difference or the force of suggestion went beyond the limits of the apparatus, and in the charts dotted curves are given for these cases. GILBERT states that the column of mean values does not give the quite correct result unless it be considered in relation to the column of percentages just mentioned. This statement would be true if the mean value used had been the arithmetic mean but is not true for GILBERT's results as the mean value used was the median. As explained on p. 32 the separate values influence the median only as being above or below it. The median child remains just the same whether an extreme child exceeds the recording power of the apparatus or not. The columns of means and their curves are thus completely correct in themselves. The columns of percentages of no-discrimination give really another representative value of no particular use in itself but quite important when compared with the medians as indicating the form of the frequency-curve and the even course of the curve of results according to age. In this respect it is as important as the mean variation.

The column of these percentages, although having no influence on the median, does have an influence on the mean variation. The mean variations are all too small by a quantity ζ following the law $\zeta = f(P)$. If the positive and negative mean variations had been calculated for the $(100 - P)\%$ of the cases separately, a deduction on the assumption of (11), p. 12, would have rendered it possible to

calculate the actual mean variation for the whole number independent of the limitations of the apparatus. The gain, however, would have been incommensurate with the labor; the mean-variation-curves would resemble those actually given and would simply be steeper at the left.

The columns headed *PB* and *PG* give us the means of answering the question as to when the difference between the boys and girls recorded for any given age is to be considered as a true difference between the sexes or as merely the result of the finite number of cases considered.

The method is as follows. Let the percentage of the total n boys within the limits of the test be $p=(100-PB)\%$ and of those beyond be $q=PB\%$; likewise for n girls let the percentages be $p'=(100-PG)\%$, $q'=PG\%$. According to a well-known theorem[1] we can assert with a probability of $\Phi(\gamma)$ that the two classes are different, provided

$$p-p' > \pm\gamma\sqrt{\frac{2pq}{n}+\frac{2p'q'}{n'}},$$

where $\Phi(\gamma)$ is the function expressed on p. 20.

I have tested some specimen cases in this way and find that for these three tests it cannot be asserted in many cases with a practical certainty of $\Phi(\gamma)=0.999978$ that there is a real difference between the two sexes.

Another test is furnished by BAYES's theorem used on p. 38. This theorem can be applied to all the tables.

In a like manner the differences between successive ages can be tested.

[1] Illustrated in LEXIS, Einleitung in die Theorie der Bevölkerungstatistik, 103, Strassburg 1875.

EXPERIMENTS ON THE HIGHEST AUDIBLE TONE,

BY

E. W. Scripture and Howard F. Smith.

The highest audible tone, or the upper limit of pitch, is that tone at the extremity of the series of tones arranged according to pitch beyond which any rise in frequency of vibration fails to produce a sensation of tone. The highest audible tone is defined psychophysically by the frequency of the physical vibrations corresponding to it. The term "vibration" is understood to mean one complete pendular oscillation including both phases. The highest audible tone has been differently determined by various observers: Sauveur, 6 400: Chladni, 8 192 ; Wollaston, 25 000 ; Savart, 24 000 ; Despretz, 36 864 ; Blake, 40 000 to 60 000.

The great discrepancy in the results is usually said to have been due to the imperfections of the apparatus employed. There is no need, however, of this assumption, as there is a source of variation quite sufficient to explain the discrepancy ; it is unquestionable that these men worked with tones of different intensities.

Even in the very latest experiments the factor of intensity has been generally overlooked. Savart was the first to observe that the highest audible tone was different for different intensities. Rayleigh[1] took care to keep his tones of approximately the same intensity. Blake, who used a succession of steel bars of varying length, produced the tone by a pendulum-hammer swinging over a graduated scale, thus insuring a nearly uniform stroke and correspondingly uniform intensity of tone. These experiments, however, went no farther than to secure a constant intensity.

The highest audible tone requires in each case a measurement of intensity as well as of pitch in order to complete its determination. It thus becomes important to inquire how the pitch of this tone depends on the intensity, or, in other words, to determine what the highest audible tone is for each intensity.

Apparatus.

The first step in solving this problem was the selection of an apparatus giving tones of the required pitch, but so arranged that the

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[1] Rayleigh, *Acoustical observations; Very high notes*, Phil. Mag., 1882 (5) XIII 344.

intensity could be kept constant at any desired point and could be readily and accurately varied. Tuning forks were not used because the range for the highest audible tone is so wide that a very large number of forks would be required, and also because they do not admit any accurate regulation of intensity. The rods of KÖNIG with pendulum-hammer are in some respects better than the forks but are not very easy to manipulate in rapidly conducted tests, such as are necessary in order not to fatigue the observer; moreover, the sound of the impact of the hammer cannot fail to be a disturbing element in the experiment. Neither the forks nor the rods give a sustained tone, but one of rapidly decreasing intensity.

The GALTON-KOENIG whistle was selected as the most reliable and readily manipulated instrument for the tests proposed. It gives a sustained tone as long as the blast of air continues; the pitch of the tone is readily and accurately varied. The whistle consists of a brass tube, about 7^{cm} in length with a cap screwing over one end. To this cap there is attached an accurately fitting piston, which moves inside the tube as the cap is screwed upward or downward. The outside of the tube is marked with a longitudinal scale the unit of which is 1^{mm}, the length of the scale being 12^{mm}. The screw is so arranged that one complete turn of the cap carries the piston a distance of 1^{mm}. The upper rim of the cap is divided into ten divisions, each one representing a movement of the piston through 0.1^{mm}. These divisions being very large, the sub-divisions of 0.01^{mm} can be obtained by the eye without error. The maker's graduation was verified to 0.01^{mm}. The whistle is blown by a current of air forced into the bottom of the tube; near the bottom there is a narrow slot extending across one-half of the circumference. The whistle is thus a closed labial pipe and the tone produced will be determined by

$$n = \frac{v}{4l},$$

n being the number of vibrations, v the velocity of sound and l the length of the pipe. The velocity of sound in dry air at 0° C. is generally given as 330.7^{m}; for the temperature of $t°$ it will be

$$330.7\sqrt{1 + 0.00367\,t}.$$

The average temperature of the room used can be taken as 20° C.; as the temperature was not recorded we can assume ± 1° C. as the limit of fluctuation during a set of experiments. This gives $v = 342.525^{m}$ with a mean error of 5 per cent. (estimated). As the actual

mean variation for a set of results seldom exceeded 5 per cent. it may well be supposed that the constant of precision of the measurements was in this case determined by technical errors and not by psychological variations. In future experiments it will be necessary to maintain the room at an even temperature during each set of experiments; and to calculate the pitch with the appropriate value of v for each change, possibly also to be on guard against sudden barometric changes.

It may seem remarkable that we should have neglected to record the temperature of the room. We give the following as reasons: 1. we did not expect after the elimination of the error of air-pressure to find the psychological sources of error smaller than the technical ones; 2. the temperature of the air has not been regarded in previous experiments; 3. it is not the custom of psychological laboratories to pay attention to the psychological and instrumental errors due to changes in temperature.

Several means of blowing the whistle have been employed. GALTON[1] used a single rubber bulb. ZWAARDEMAKER[2] used a funnel with a rubber membrane stretched across the large opening, the smaller end being connected with the whistle by a rubber tube; the funnel was depressed through a constant distance. These methods are none of them strictly reliable. The pressure is intermittent and cannot be accurately regulated. The only possible method of obtaining a current of air of constant intensity, seems to be by use of a rotary-fan blower. This method has been previously described and tested.[3] The source of power used by us was an electric motor run by the city-current; there were no perceptible fluctuations in speed. The fan-wheel of the blower made from 13 000 to 15 000 revolutions a minute. The blast was carried by a rubber hose into a room in another part of the laboratory; thus all noise from the machinery was avoided. The hose led to a rubber tube, in which was a stop-cock. The rubber tube ended in a glass T-coupling with a rubber tube on each end of the cross-arm. One of these led to the whistle, the other to a water-manometer. The manometer scale was graduated to millimeters; the height of the column of water gave the

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[1] GALTON, *Whistles for audibility of shrill notes*, Inquiries into Human Faculty, 88, New York 1883.

[2] ZWAARDEMAKER, *Der Umfang des Gehörs in den verschiedenen Lebensjahren*, Zt. f. Psych. u. Phys. Sinn., 1894 VII 10.

[3] SCRIPTURE, *A constant blast for acoustical purposes*, Am. Jour. Psych., 1892 IV 582.

pressure of the blast of air supplied to the whistle. The pressure was regulated by the stopcock. In this manner a constant blast of air could be maintained for any given time and the intensity could be varied at will. The fluctuations of pressure as indicated by the manometer did not exceed 1 per cent. Experiments were made with five different pressures, 50^{mm}, 100^{mm}, 150^{mm}, 200^{mm} and 250^{mm}. Within the limits of accuracy of 5 per cent. with which the experiments were conducted, the intensity of the vibratory movement could be considered as varying in direct proportion to the pressure of the blast.

In some of the earlier experiments we used a foot-blower as a bellows. Even the best bellows cannot be accurately regulated; a foot-blower still less so. The following mean variations are from experiments on trustworthy observers. With the foot-blower we find, for example, mean variations of 10%, 14%, 5%, 9%, 8%, 13%; with the rotary-fan blower, 5%, 9%, 2%, 7%, 4%, 2%. The records are not on the same observers in the two cases but the difference is sufficient to indicate quite a gain in accuracy by using the rotary blower.

The whistle was started near 0, i. e. above the upper limit of pitch, and the cap was gradually unscrewed until the observer detected a tone. The reading of the scale was observed at this point. The cap was unscrewed a little further to make sure that a good musical tone was heard; then it was screwed up again, stopping at the instant the observer lost the tone. The two readings were noted down in separate sets, D and A. This was repeated five times, making 10 records with the given intensity. Sets of records were made in succession with the five pressures, beginning at 50^{mm}. After a rest of about 3 minutes another series of 50 records was made but the pressures were used in the reverse order. This reversal of the order of the pressures eliminated the error of fatigue, if there was any.

It will be noticed that the method differed from that used by previous observers, being the method of regular variation.[1]

The execution of the experiments was in charge of H. F. Smith who is responsible for the care exercised. The setting up of the blower, shafting and belting was done by the laboratory mechanic, J. H. Hogan, who controlled the running of the machinery during the experiments. Rubber belting was used, but owing to its inferior flexibility it should in future be avoided for small pulleys like that

[1] SCRIPTURE, *On the method of regular variation*, Am. Jour. Psych., 1891 IV 577.

SCRIPTURE, *Ueber die Aenderungsempfindlichkeit*, Zt. Psych. Phys. Sinn., 1894 VI 472.

on the blower ; with the high speed used it has been ignited by the slipping. The whole arrangement of apparatus was supervised and inspected by E. W. Scripture.

COMPUTATION.

The median value was taken for each single set of five of the same kind. For the result descending or for the result ascending the two medians were weighted in the usual way inversely as the square root of the mean variations and were then averaged. For the final result the four medians were weighted and averaged.

As the use of the median is new, all the results were calculated also in the usual way to obtain the arithmetical mean. The components were weighted in the regular way.

The computation was done by E. W. Scripture with the aid of CRELLE's, Rechentafeln. There were in all 120 sets of 5 results each. The median of each set was first recorded, then the error of each result from its median was written underneath it. The average of each set and the error of each result were likewise computed and were written beside the median and its errors. It was soon noticed that the average seldom differed much from the median ; this was used as a check on the computation of the average, the whole computation being repeated in case of much divergence. It was also noticed that, given the median and the average, the errors from the average could be calculated from the column of errors from the median ; for the latter half of the work this was done by the amanuensis while the computer calculated the errors directly. As a credential for the reliability of the computation, it can be said that only one mistake in the calculation of the errors was found in the results verified in this way. The mean errors of each of the 240 sets of five errors were computed according to the formula on pp. 18, 24, the division by 4.5 being performed from the multiplication table for 45. The square root of each mean error was taken from a table of square roots. The reciprocals for weights were written as decimals to two places.

INFLUENCE OF INTENSITY.

The results are given in table I. Although the results were calculated into vibrations, they were allowed to stand in the table as hundredths of a millimeter in order to avoid the impression of a false degree of accuracy which arises when 0.01^{mm} is turned into a whole number with zeros at the end. In the curve (fig. 33) the frequencies are given for the sake of comparison.

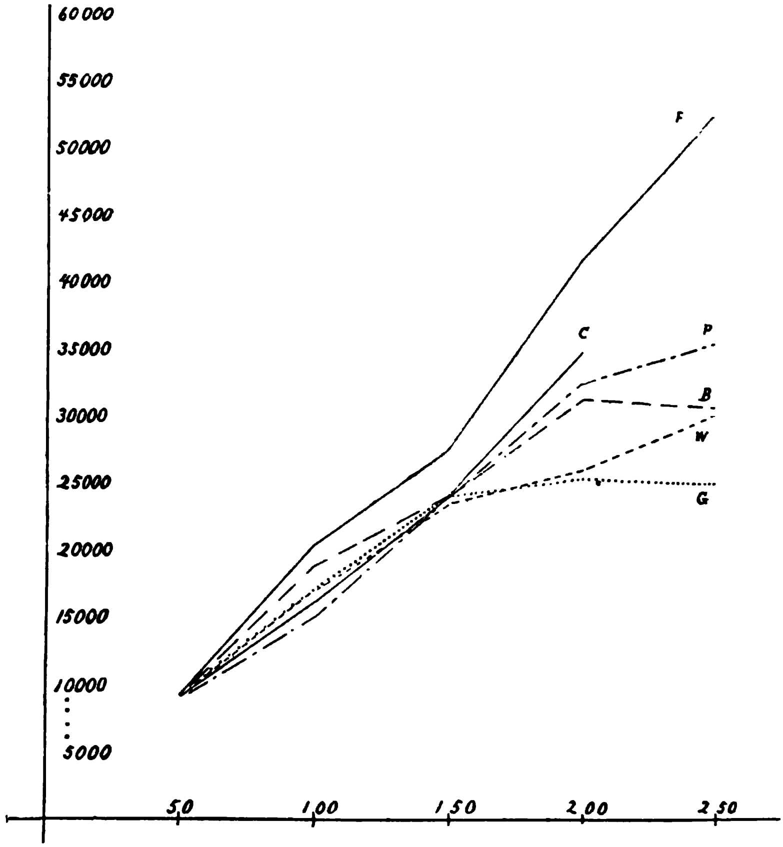

FIG. 33.

TABLE I.

Unit of measurement, 0.01mm.

	50mm.	100mm.	150mm.	200mm.	250mm.
Comstock	932 928	541 543	361 348	247 245	
Bishop	916 924	464 462	368 364	276 277	280 279
Waters	955 943	524 514	381. 377	335 328	287 291
Persons	938 938	566 561	365 360	265 268	242 249
Gosling	923 917	527 527	364 366	339 337	342 340
Furbish	907 905	424 425	317 320	207 202	163 174

Large figures, computation with medians.
Small figures, computation with averages.

1. The general result for all observers indicates that the pitch of the highest audible tone varies directly and almost proportionately with the intensity. The deviation from exact proportionality does not exceed the mean variation of the separate observers from the final averages. The curves of results agree very closely for all observers except for the pressure 250^{mm}. This disagreement may be due to the very great and almost painful intensity of the tone.

2. The lowest results reach almost to CHLADNI's and the highest almost to BLAKE's, suggesting differences in intensity as the possible sources of discrepancy of previous results.

3. How far the upper limit can be pushed with still greater intensity, it is impossible to say. With a pressure of 250^{mm} of water the tone of the whistle is already very painful and fatiguing.

INFLUENCE OF DIRECTION.

Experiments descending from 0 of the scale, i. e. from high to low pitch, or from silence to sound, alternated with those from low to high, or from tone to silence. The differences between the two are shown in table II. In order to detect any influence of fatigue the separate pairs for a given pressure were not united.

TABLE II.

Unit of measurement, 0.01^{mm}.

	50^{mm}.	100^{mm}.	150^{mm}.	200^{mm}.	250^{mm}.	250^{mm}.	200^{mm}.	150^{mm}.	100^{mm}.	50^{mm}.	R.
Comstock	0	+23	+20	+29				+23	+39	+38	+23
	0	+ 37	+ 58	+ 43				+ 50	+ 30	+ 68	
Bishop	+42	+55	+21	+65	+55	+75	+50	+45	+41	+46	+50
	+ 43	+ 57	+ 28	+ 53	+ 53	+ 83	+ 47	+ 36	+ 26	+ 46	
Waters	+11	+46	0	+ 2	+ 5	− 2	+35	+28	+ 7	+24	+ 9
	+ 25	+ 16	− 14	+ 25	+ 3	+ 11	+ 28	+ 2	+ 15	+ 17	
Persons	+40	+25	+26	+17	+10	− 3	+43	+23	+36	+18	+24
	+ 43	+ 14	+ 28	+ 16	+ 11	− 6	+ 30	+ 34	+ 13	+ 19	
Gosling	+37	+25	+21	+ 2	+28	+26	+ 5	+32	+31	+24	+26
	+ 16	+ 13	+ 19	+ 13	+ 22	+ 13	+ 9	+ 34	+ 28	+ 17	
Furbish	+20	−14	−43	+28	+32	− 6	+14	+36	+16	+36	+18
	+ 22	+ 7	− 41	+ 30	+ 19	− 6	+ 34	+ 30	+ 8	+ 22	

The figures give the differences between results descending and ascending, D—A.
Larger figures, differences between medians.
Smaller figures, differences between averages.

The figures prove conclusively that the highest audible tone when proceeding from silence to tone is much higher than the highest audible tone from tone to silence. The column R gives the median difference for each person for the various pressures. The average

difference was not calculated. An inspection of the table shows that the averages run along closely with the medians; the calculation of the averages would require considerable labor and would add nothing to our knowledge.

The table makes evident that, within the limits of accuracy employed, D—A is not a function of the intensity.

Fatigue.

Single sets of experiments sometimes showed considerable differences whether taken at the beginning or at the end of the series. To determine the amount of fatigue the difference between the first pair and the last pair for each pressure was computed. Thus a table was formed giving the differences between the 1. and 10. pairs, 2. and 9. pairs, 3. and 8. pairs, 4. and 7. pairs, 5. and 6. pairs. The values showed such irregularity that it can not be said either that fatigue raises the upper limit or lowers it.

In similar manner some of the mean variations were compared. The result was the same; it cannot be said that fatigue influences the regularity of judgment.

The attempt was made to fatigue the ear directly. After the complete set of experiments was finished with Bishop, the whistle was blown steadily for 45 seconds at 200^{mm} pressure and then 10 records were taken at that pressure. The change of upper limit was not sufficient to warrant any conclusion. This line of experiment lay somewhat aside from the problem and was not extended.

Miscellaneous observations.

A phenomenon, closely related to fatigue, appeared in the oscillatory change from sound to silence. If the whistle was kept stationary at the pitch where the tone had just been lost, the tone would alternately be heard and lost again. The experience is similar to the phenomenon of fluctuation of attention with weak sensations. Another fact noticed was that even above where the tone could no longer be heard, an indefinite, somewhat painful sensation was felt in the ear.

It may not be uninteresting to note certain questions arising concerning the functions of the middle and internal ear. Helmholtz's piano-string theory of the function of the cochlea being accepted, does the energy required to arouse the shorter resonating membranes increase as the pitch of the membrane increases? If so, why should there be the oscillating fluctuation in the hearing of the tone as just

noted? The question of the end-organs being left aside, might not the abilty of accommodation by the tympanum, as determined by the action of the *M. tensor tympani* be the determining factor for the highest audible tone? The extent of the reflex-action of a muscle depends to some degree on the intensity of the stimulus affecting the sense-organs. The impulse to accommodation proceeding through sensory and motor centers might be weaker for weak sounds, the tympanum would be less tightly stretched and as the pitch increased the limit of accommodation would be reached sooner than with louder sounds. On the other hand loud sounds would produce a much greater tension and therefore a higher accommodation. If this hypothesis is justifiable, the highest audible tone would be a matter of tympanic accommodation. The oscillations mentioned would correspond to the oscillatory fatigue and recovery of the nervous centers regulating muscular effort.

In this connection it may be suggested that the gradual falling off in the pitch of the highest audible tone with advancing age[1] may be due, 1. to the gradual loss of function of the resonating organs of the cochlea, proceeding from those of higher pitch downward; 2. to the gradual obtuseness of these organs, rendering them functionless for a given intensity but capable of answering to greater intensities; or, 3. to a gradual decrease in the power of accommodation. The problem cannot be settled till ZWAARDEMAAKER's experiments are repeated with different intensities.

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[1] ZWAARDEMAAKER, *Der Umfang des Gehörs in den verschiedenen Lebensjahren*, Zt. Psych. Phys. Sinn., 1894 VII 10.

ON THE EDUCATION OF MUSCULAR CONTROL AND POWER,

BY

E. W. Scripture, Theodate L. Smith and Emily M. Brown.

In an article on the course of muscular training Fechner[1] recorded the number of times day after day that he was able to raise two dumb-bells, about 9½ lbs. each, once a second from his side to over his head. The records extended over sixty days in succession. They show a steady general gain with small oscillations, the general course of the curve representing the increase of power owing to practice and the oscillations showing the conflicting effects of fatigue. The final conclusion, as stated by Fechner, is that during the first 14 days there were no permanent effects of practice visible, that up to the 40. day there was a gradual gain and that with the 41. day there was a great gain which increased rapidly with great oscillations till the 55. day, after which there was a sudden fall.

Volkmann[2] made experiments on the education of the fineness of space-discrimination as judged by the skin, using Weber's compass in the usual way. These experiments, however, are not quite comparable with Fechner's as each series was made at a single sitting. Volkmann's two series of experiments on sight extended over 12 days and gave curves similar in form to his touch curves. Volkmann's curves resemble Fechner's if we omit the flat part of slow increase at the beginning on the supposition that both skin and eye have already received their early training. In the same article Volkmann relates experiments showing that practice of the finger-tip of the left hand increases the fineness of touch of the finger-tip of the right hand but does not increase that of the left fore-arm. Further experiments show that practice on the third phalanx increases the fineness on the first phalanx. Thus, training of one portion of the body trains at the same time the symmetrical part and also neighboring parts.

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[1] Fechner, *Uber den Gang der Muskelübung*, Ber. d. k.-sächs. Ges. d. Wiss., math.-phys. Kl., 1857 IX 113.

[2] Volkmann, *Ueber den Einfluss der Uebung auf das Erkennen räumlicher Distanzen*, Ber. d. k.-sächs. Ges. d. Wiss., math.-phys. Kl., 1858 X 38.

FECHNER[1] relates an observation by WEBER on the ability to write with the left hand obtained by learning with the right hand. FECHNER states that practice in writing the figure 9 backward with the left hand frequently caused him involuntarily to write the 9 backward when he used the right.

These observations seemed of sufficient importance to justify a further inquiry regarding the general law of education followed by our muscular abilities and also regarding the possibility of what may briefly be called "cross-education." It proved most convenient to make experiments on muscular control and on muscular power; the former were carried out by Miss Smith, the latter by Miss Brown.

MUSCULAR CONTROL.

In undertaking the experiments on muscular control two questions were proposed: 1. Can steadiness of movement be increased by practice? 2. If so, is such increase confined to the muscles immediately trained or, as in the case of discriminating sensitiveness of the skin, are the corresponding muscles in the opposite half of the body affected.

The apparatus used for these experiments consisted of a Brown & Sharpe twist-drill gauge, 2^{mm} thick, having a series of 60 holes varying in size from 0.0400 in. to 0.2280 in. (0.1160^{mm} to 5.7912^{mm}). This was fixed on a board in a vertical position and connected with one pole of a battery. From the other pole a flexible connector led to a light rod 75^{cm} long in the end of which a needle was inserted. An electric bell introduced into the circuit recorded any contact of the needle with the gauge-plate.

In the first experiments the method was tried of putting the needle without touching the plate into as many successive holes of decreasing size as possible, ending the trial at the first error. Although the results indicated a marked increase of steadiness in both hands, the mean variation was so great, owing largely to the element of fatigue which limited the number of experiments taken at one time, that they were thrown aside as worthless. After an interval of three weeks, during which the results of the previous training had disappeared, the experiments were resumed. This time the measure of accuracy was the ability to insert the needle into a single hole 0.1285 in. (3.2639^{mm}) in diameter. The vertical metal plate

[1] FECHNER, *Beobachtungen, welche zu beweisen scheinen, dass durch die Uebung der Glieder der einen Seite die der andern zugleich mit geübt werden*, Ber. d. k.-sächs. Ges. d. Wiss., math-phys. Kl., 1858 X 70.

containing the hole was placed directly in front of the observer; the right fore-arm was rested on the edge of the table; the stick was grasped like a pencil and by a steady movement of the hand and wrist the metal point was inserted in the hole. Any contact of the point against the side of the hole was counted as an error. The per cent. of successful insertions was considered the measure of accuracy. Since the completion of the experiments a new apparatus (fig. 34) has

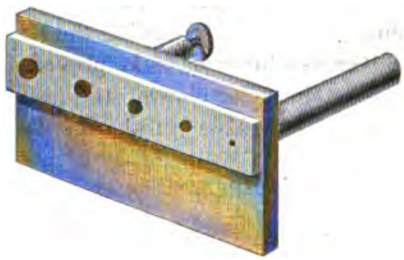

FIG. 34.

been invented especially for the purpose. It represents the result of previous experience and will be used for future work. It consists of a flat block of hard rubber supported vertically by a rod. On the face of the block is a strip of brass in which there are five hard rubber circles, 1^{mm}, 2^{mm}, 3^{mm}, 4^{mm} and 5^{mm} in diameter. Electrical connection is made by a binding-post at the back. The edges of the circles are flush with the brass. The object is to touch the rubber circle with the metal point by a single steady movement. Sufficient unsteadiness of the hand will cause the point to touch the metal, whereupon the alarm is rung. With the same circle the steadiness of the hand can be considered to be directly proportional to the per cent. of successful trials. The movement of the hand is guided by sight.

The experiments were all made by Miss Smith with the drill gauge before the invention of the new apparatus. The first set consisted of 20 experiments with the left hand; the result was 50 per cent. of successful trials. Immediately thereafter 20 experiments were made with the right hand, with a result of 60 per cent. of successful trials. On the following day and on each successive day two hundred experiments were taken with the right hand, the same conditions in regard to time, bodily condition and position in making the experiments being maintained as far as possible. The percentage of successful trials ran as follows: 61, 64, 65, 75, 74, 75, 82, 79, 78, 88. The increase in accuracy is represented in the curve in fig. 35.

On the 10. day the left hand was tested with twenty experiments

as before, with 76 per cent. of successful trials, thus showing an increase of twenty-six per cent. without practice in the time during which the right hand had gained as shown by the figures above.

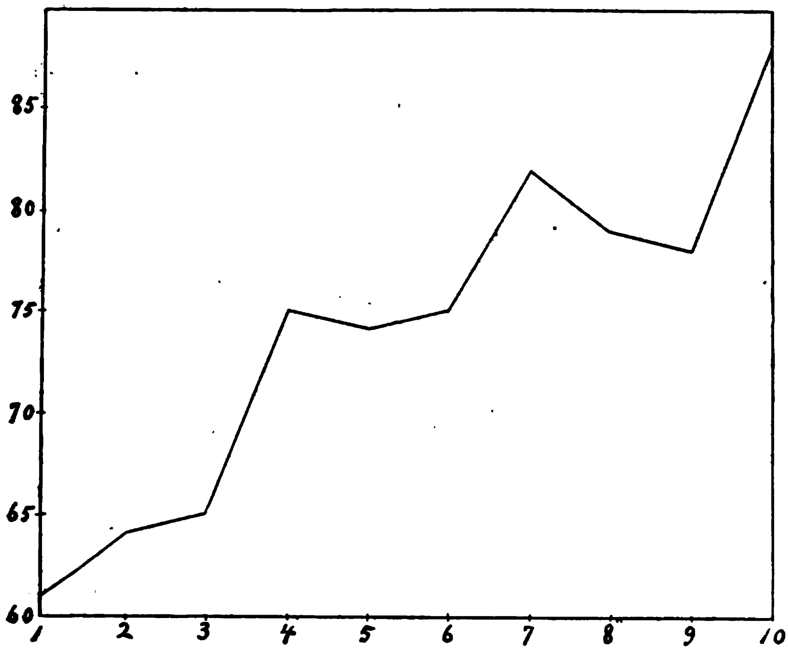

FIG. 35.

That the increase of steadiness was not due to mere training of the muscles is shown by the increase of steadiness in the unpractised left hand. That it was due to a training of the attention seems to be indicated by the following facts. 1. After a week's practice it was possible by a special effort of attention to insert the needle into the hole successfully for any given ten times. 2. Any distraction of attention due to noises or other disturbances invariably lowered the per cent. of steadiness. 3. Either bodily or mental fatigue lowered the result.

As to the effect of different directions of attention : concentration upon the muscular movement to be performed was unfavorable, but fixation of attention upon the objective point to be reached by the needle was productive of the best results. Fatigue of the muscles of the eye was a more noticeable result than fatigue of the muscles directly practised. To obviate this it was necessary to close the eyes for a few seconds between each series of ten experiments.

From the results of these two thousand experiments the following conclusions seem justified.

1. Steadiness of movement can be increased by practice.

2. This increase of steadiness is not limited to the control of the muscles immediately trained but affects the control of the corresponding muscles on the opposite side of the body.

3. This training seems to be of a psychical rather than of a physical order and to lie principally in steadiness of attention.

Muscular power.

The experiments on the increase of muscular power due to practice were made by Miss Brown. The apparatus consisted of a mercury dynamometer with a rubber bulb. The mercury was contained in a closed bottle from the bottom of which rose an open vertical glass tube. Another tube from the bottle led to the bulb by means of rubber tubing. The bulb and the space in the bottle were filled with water, thus giving water-transmission of the pressure. By means of a Y-tube, a stopcock and an adjustable reservoir of water the mercury could be readily adjusted to the zero-point. The graduation on the scale back of the mercury tube was in inches. The person experimented on was seated; the bulb was grasped in the hand and was squeezed as strongly as possible. The height attained by the mercury was observed; after about a minute employed in making the record and resting, the experiment was repeated. Ten experiments were made on each occasion excepting the 16., when only 6 were made. The first set was made on 7 III 1894 with the left hand; the average was 29.6 inches. Immediately thereafter a set was made with the right hand. On following days the experiments with the right hand were repeated with results as given in the table.

Date, March.	Pressure in inches of mercury.	
	L	R
7	29.6	28.8
8		33.7
9		35.6
10		36.6
12		40.9
14		44.7
15		47.0
16		48.8
20	42.3	48.6

Immediately after the experiments with the right hand on the 20. they were again made with the left hand which had not been used in the mean time.

The results show a steady increase in the muscular power of the right hand due to direct practice and also an increase in the power of the left hand due to what we might call "indirect practice."

During the progress of the experiments Miss Brown exercised both arms with dumb-bells on three successive evenings for a short time. The muscles so intensely exerted in the dynamometer measurements are not very strongly called into play in the dumb-bell exercise. Nevertheless we prefer not to lay weight on the actual form of the law of increase in power of the right hand but to confine the statement of the result to the single fact that practicing the right hand develops the left also.

A PSYCHOLOGICAL METHOD OF DETERMINING THE BLIND-SPOT,

BY

E. W. Scripture.

No greater misfortune could happen to psychology than to have it supposed that its measurements were physical or physiological rather than purely mental. The phenomena of consciousness are not unattainable things situated at the central termination of nerve-paths; they are directly given, purely mental facts known to every savage or child regardless of the existence of brain or nerves or sense-organs. As purely mental facts we can measure them by one another with an accuracy rapidly approaching that of physics. As mysterious processes resulting from a complicated succession of physiological changes, we can do nothing with them.

The treatment of the blind-spot is a striking illustration of the difference in the physiological and the psychological points of view. In Helmholtz's *Physiologische Optik* the blind-spot is treated as a physiological matter and is used to prove that the optic nerve is not directly sensitive to light. The first step, however, is mental; in our field of vision we find a constant spot on which we are blind. We may know nothing about the optic nerve or the function of the eye, but the fact of blindness can easily be made apparent. It is my object to show how this blind-spot can be measured as a fact of consciousness without the assumptions of the passage of rays into the eye, etc. After such psychological measurements have been made, the results can be compared with the position of the *Papilla nervi optici* in relation to the optical axis of the eye and its non-sensitiveness can be deduced.

The apparatus required consists of a board with a straight side (drawing-board), a T-square, a draughtsman's triangle or a straight piece of wood or metal to be used as a sliding piece against the square, a millimeter-scale and three pins. Two pins are pounded into the board close to the straight edge; the head is fixed so that one is seen exactly behind the other. The other pin is fastened into the sliding piece or triangle and is moved from one side in a line at

right angles to the line of the two pins until it just disappears; this is the edge of the blind-spot. The T-square is now moved nearer or farther away and the measurement repeated. The results for the left eye are indicated in the diagram.

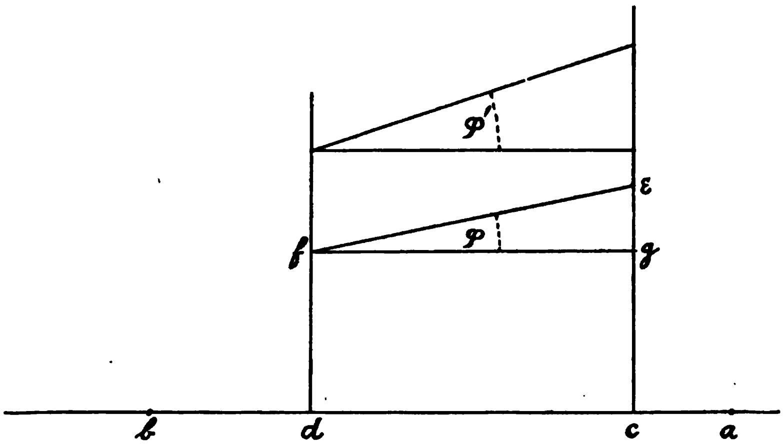

FIG. 36.

The two vision-pins are at any points a and b. The edge of the T-square is put at c; at the point e the movable pin disappears. When the T-square is placed at d, the pin disappears at f. What is the angular distance of the edge of the blind spot from line of regard? Drag fg parallel to ab; this gives $gfe = \varphi$ as the angle to be determined. Since $ge = ce - df$, and $gf = cd$, we get at once

$$\tan \varphi = \frac{ce - df}{cd}$$

If we thus determine φ for the inner edge and in a like manner φ' for the outer edge of the blind-spot, the angular diameter of the blind-spot is $\varphi'' = \varphi' - \varphi$.

TESTS OF MENTAL ABILITY AS EXHIBITED IN FENCING,

BY

E. W. SCRIPTURE.

The visit of several expert swordsmen to Yale furnished the opportunity for some experiments on their rapidity and accuracy in some of the fundamental movements of fencing.

The first experiment included a determination of the simple reaction-time and of the time of muscular movements. The fencer stood ready to lunge, with the point of the foil resting to one side against a metal disc. A flexible conducting cord, fastened to the handle of the foil, hung in a loop from the back of the neck. A large metal disc was placed against the wall directly in front of the fencer at a distance of 75^{cm}. Just above this disc was a small piece of paper which could be moved by an operator, standing a distance away. A movement of the paper was the signal upon which the lunge was executed. The movement of the paper was accomplished by a single movement of an electric switch. The spark-method of recording[1] was so arranged that the primary circuit passed through the electric switch, a spark-coil, the flexible conducting cord, the foil and either one of the two discs. Every make and break of this circuit made a spark-record on the drum. As long as the foil rested against the small disc the current was closed. The movement of the switch broke the circuit for an instant, making a record of the moment of stimulus. The first movement of the foil broke the circuit at the small disc, making a record of the moment of reaction. The striking of the foil against the large disc made a third record. The time between the first and second records gave the simple reaction-time; that between the second and third gave the time of movement through the given distance. About 10 experiments were made on each person.

In the second experiment there was one piece of paper each above, beside and below the direction of the foil. The point of the foil

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[1] BLISS, *Researches on reaction-time and attention*, Stud. Yale Psy. Lab., 1892-1893 I 1 (8, 14).

rested against the small disc. The movement of any one of these was the signal for a corresponding movement of the foil. The papers were moved in irregular succession. Acts of discrimination and choice were thus introduced into the reaction-time. The movement of any one of the pieces of paper and of the foil away from the disc gave records as before. The time required can be called the reaction-time with discrimination. About 10 experiments were made on each person.

The last experiment consisted of lunging at the center of a paper target 8^{cm} in diameter. The average distance of the seven best lunges was taken.

The persons experimented upon consisted of Dr. Graeme Hammond, Dr. Echverria, Dr. P. F. O'Connor and Mr. Shaw, all expert amateur fencers, A. Jacobi, master of arms of the New York Athletic Club, Prof. Ladd, formerly practised in fencing, and Prof. Williams, with no knowledge of fencing.

The results were:

1. Simple reaction-time: Echverria, 173^{σ}; Williams, 186^{σ}; Hammond, 187^{σ}; Ladd, 225^{σ}; Jacobi, 231^{σ}; Shaw, 233^{σ}; O'Connor, 256^{σ}.

2. Time of muscular movement involved in the lunge through 75^{cm}: Jacobi, 267^{σ}; O'Connor, 294^{σ}; Echverria, 306^{σ}; Shaw, 322^{σ}; Hammond, 323^{σ}; Ladd, 517^{σ}; Williams, 568^{σ}.

3. Reaction-time with discrimination: Hammond, 221^{σ}; Ladd, 237^{σ}; Williams, 254^{σ}; Jacobi, 289^{σ}; Echverria, 304^{σ}; Shaw, 357^{σ}; O'Connor, 362^{σ}.

4. Average distance of seven best lunges from center: Shaw, 18^{mm}; Hammond, 20^{mm}; Ladd, 21^{mm}; O'Connor, 22^{mm}; Jacobi, 24^{mm}; Echverria, 23^{mm}; Williams, 36^{mm}.

The experiments probably derive their chief value as calling attention to the experimental study of the psychological elements involved in games, sports, gymnastics and all sorts of athletic work. Without experimenting on large numbers of fencers and others, I would not attempt to make any quantitative comparisons between the two. The following qualitative conclusions seem, however, to be fully justified.

1. The possibility of analyzing fencing movements into their mental and bodily elements, and of measuring these elements, has been proven.

2. The average fencer is not quicker in simple reaction (where a few mental elements are involved), than a trained scientist, and neither class shows an excessive rapidity.

3. When once the mind is made up to execute a movement, fencers are far quicker in the actual execution. In rough figures, it takes them only half as long as the average individual.

4. As the mental process becomes more complicated, the time required by the average fencer is greater than that required by a trained scientist. The shortest time of all, however, is that of Dr. Hammond, whose mental quickness has probably been developed in some other way.

5. The general conclusion seems to be that fencing does not develop mental quickness more than scientific pursuits, but it does develop to a high degree the rapidity of executing movements. It would be important to determine if this holds good of the other sports and exercises, or if some of them are especially adapted to training mental quickness.

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8 Oct., 1898.

MEASUREMENTS OF ILLUSIONS AND HALLUCINATIONS IN NORMAL LIFE,

BY

C. E. SEASHORE, PH.D.

PART FIRST.

ILLUSIONS OF WEIGHT: INFLUENCE OF KNOWLEDGE OF SIZE ON JUDGMENT OF WEIGHT.

When an object lifted is found heavier than was expected, it is overestimated, and when it is found lighter than was expected, it is underestimated. The strongest and most frequent illusion of this kind in normal life is perhaps that which is caused by our accustomed associations between the properties of size and weight of objects. The aim in Part First of this research is to investigate the nature and extent of the illusions of weight as caused by knowledge of the size of the body lifted. Illusions of weight from other sources are incidentally considered. The experiments were made between October, 1893, and May, 1895.

The problem is a development from a test made by GILBERT[1] on the suggestive force of size on judgment of weight. DRESSLAR[2] has made a statistical study of the same illusion, and it had previously been noticed and subjected to experiment by others.[3] Since the present experiments were completed, a monograph by GRIFFING[4] has appeared, in which he touches upon the same illusion in so far as it is connected with the sense of impact and pressure; FLOURNOY[5] also reports a very popular experiment on the same illusion.

[1] GILBERT, *Researches on the mental and physical development of school children*, Stud. Yale Psych. Lab., 1894 II 43–45, 59–63.

[2] DRESSLAR, *Studies in the psychology of touch*, Am. Jour. Psych., 1894 VI 313.

[3] CHARPENTIER, *Analyse de quelques éléments de la sensation de poids*, Archives de Physiol., 1891 (5) III 126.

MÜLLER and SCHUMANN, *Ueber die psychologischen Grundlagen der Vergleichung gehobener Gewichte*, Archiv. f. d. ges. Physiol. (Pflüg.), 1889 XLV 37.

[4] GRIFFING, *On the sensations of pressure and impact*, Psych. Rev., 1895 II Suppl. I.

[5] FLOURNOY, *De l'influence de la perception visuelle des corps sur leur poids apparent* L'Année Psych., 1894 I 198.

First series of experiments: Influence of size on judgment of weight when size is estimated by direct sight.

Two sets of cylindrical blocks 31^{mm} in length were made of brass tubing with hard-rubber ends. An additional hard-rubber disk on each end, 2^{mm} thick and 15^{mm} in diameter, served for the place of grasping. The entire length of each block was thus 35^{mm}. In order that the appearance of the surface should not suggest any definite material, the blocks were all painted a dull, smooth black with optical varnish.

Each set consisted of 17 blocks. Set A varied in size and had a uniform weight, while Set B varied in weight and had a uniform size. The blocks in Set A varied in diameter according to a geometric series in which the regular increment is one-tenth. Those in Set B were arranged in arithmetic series according to weight with a successive difference of 5^g.

In the following account the blocks will be distinguished by the names A and B with their respective numbers in the series.

The blocks of Set A were of a constant weight, 80^g, and of diameters in millimeters as follows, beginning with the smallest: 20.0, 22.0, 24.2, 26.6, 29.3, 32.2, 35.4, 39.0, 42.9, 47.2, 51.9, 57.1, 62.8, 69.1, 76.0, 83.6, 91.9.

The blocks of Set B were of a constant diameter, 42.9^{mm}, and of weights in grams as follows, beginning with the lightest: 40, 45, 50, 55, 60, 65, 70, 75, 80, 85, 90, 95, 100, 105, 110, 115, 120.

It is to be observed that the uniform weight for Set A is the same as the weight of B (9), the middle block in Set B; and the uniform size in Set B is the size of A (9), the middle block in Set A. The limit in diameter of the extremes was determined by the size which will admit of uniform grasp.

The material of which the blocks were made was not appreciably subject to change from the ordinary variations of temperature and atmospheric condition. The weight was made accurate within a limit of 0.025^g.

The two sets were placed on a tray that had a soft, black cloth bottom, Set A being arranged in order of size and Set B in order of weight.

The observer placed himself by the table on which the tray stood in such a position that by moving back and forth he could lift any block from its place in Set B and still retain approximately the same angle of the arm and hand. He was requested to select for each

weight in Set A a corresponding one in Set B, by taking one at a time from A and placing it by the side of successive blocks in B with which he wished to compare it, lifting one at a time until he found the one in B which he thought had the same weight as the one from A. He was required to use the same hand, in the same position, and to clasp the disks of the blocks carefully between the thumb and middle finger, so that in every case the touch and the

TABLE I.

Illusion of weight when the blocks are seen directly.

A	*B*	*C*	*D*	*MV*
20.0	110.2	—22.9	+30.2	7.5
22.0	103.8	—20.9	+23.8	7.0
24.2	98.2	—18.7	+18.2	5.0
26.6	94.4	—16.3	+14.4	6.5
29.3	94.0	—13.6	+14.0	6.5
32.2	89.2	—10.7	+ 9.2	8.0
35.4	86.3	— 7.5	+ 6.3	5.0
39.0	85.4	— 3.9	+ 5.4	4.0
42.9	83.8	0	+ 3.8	6.0
47.2	80.4	+ 4.3	+ 0.4	5.0
51.9	75.6	+ 9.0	— 4.1	4.0
57.1	71.6	+14.2	— 8.9	5.5
62.8	69.0	+19.9	—11.0	6.5
69.1	65.8	+26.2	—14.2	6.5
76.0	64.2	+33.1	—15.8	6.5
83.6	61.2	+40.7	—18.8	6.0
91.9	58.6	+49.0	—21.4	6.5

A, size of the block in Set A (having a weight of 80^{g}).

B, weight of the block in Set B (having a diameter of 42.9^{mm}) chosen as equal in weight to the block of Set A.

C, number of millimeters by which the diameter in Set A differed from that in Set B.

D, grams by which the estimated weight of the block in Set A differed from its true weight; average of a total of 25 experiments on 15 persons.

MV, mean variation; to obtain the mean variation for the series each result is to be divided by 5.

grip would be constant. He was at liberty to try each block a sufficient number of times to satisfy himself, but was warned against making so many repetitions that a disturbing fatigue would set in. When the experiment was repeated on the same day, sufficient rest was allowed between each set, and the observer was always cautioned to guard against any guess-work, theory, or memory of previous judgments. The observer had no names for the blocks, but,

as he indicated his choices, the experimenter kept private record. He saw the difference of size in A and was told the difference and order of weight in B. Before beginning the experiment, he had the privilege of trying the blocks at random, and in the experiment no restrictions were made in regard to what order the A's should be taken in the set.

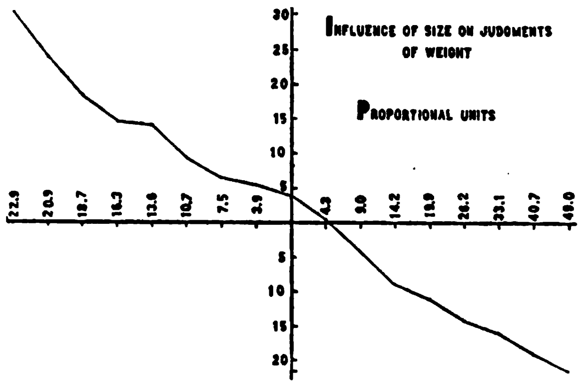

FIG. 1.

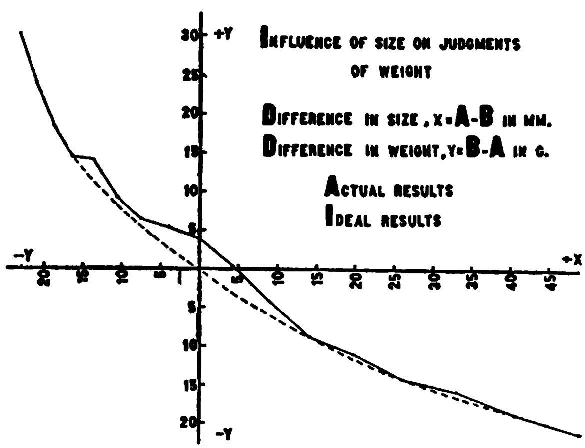

FIG. 2.

This method insures a direct measurement of the illusion caused by the influence of size in discrimination of weight. The results are given in Table I, and are graphically represented in figures 1 and 2.

In fig. 1 each successive increment of $\frac{1}{10}$ of the size is represented by one unit of the abscissa. If, according to Weber's law,

proportional increments are perceived as equal increments, the successive differences are equal. The chart thus represents the differences in diameter as perceived.

The curve, fig. 2, shows that, with slight exception, proportional increments of size produce illusions of equal absolute differences in weight.

The uniformity of the mean variation is strong evidence of the regularity of the illusion. In these 25 experiments it varies between 4^g and 8^g. The 15 observers are in such consensus that even in the case of the greatest illusion the mean variation is only 6.5^g, while for the ninth weight, where there is no illusion, the error of observation is 6^g. This fact tends to show that the influence due to size in the determination of weight within a middle range is almost as potent as an absolute difference in weight.

This regularity is further emphasized by the fact that there is not, for all these observers, a single exception as to the direction of the illusion in the first three and last six blocks in the set.

The psychological laws expressing the influence of knowledge of size on judgment of weight, within a limited range, may be formulated as follows:

1. Bodies of similar material that have the same weight, but differ in size, appear to differ in weight when compared.
2. The larger is underestimated and the smaller is overestimated.
3. The intensity of this illusion varies directly with the perceived amount of difference in size between the bodies compared.

Second series of experiments: Persistence of the illusion of weight.

A. Actual relations of weight unknown.

Having determined that the direction of the illusion was the same for all persons tried, and that it approximated the same amount, the next step was to test its persistence.

In this problem the first question to be settled was this: Does the illusion persist in spite of continued practice in attempting to gain accuracy in discrimination for weight, as long as the observer does not know the actual weight or the presence of the illusion?

Four careful observers were selected and subjected to the same tests independently, according to the same method of experimenting as in the preceding series.

Each observer tried the same complete experiment twenty times, under similar circumstances, making two experiments each day. No

suggestion as to the degree of accuracy or success was given during the progress of the experiments. The instructions emphatically expressed were : "The object of these experiments is to determine whether you can improve in the accuracy of this discrimination by practice. Do not allow yourself to be influenced in the least by memory of previous judgments or any theory of order or expected results."

In addition to the error of observation, expressed by the choice of corresponding B's for A (9), an extra test for the normal degree of

TABLE II.

Persistence of the illusion of weight when the fact of the illusion is not known.

C	D_I	MV	D_{II}	MV	D_{III}	MV	D_{IV}	MV	AD
—22.9	+22.8	2.0	+33.8	1.8	+31.8	1.7	+27.8	1.9	+28.9
—20.9	+18.5	0.8	+31.0	1.4	+28.3	1.5	+23.3	2.8	+25.3
—18.7	+15.5	0.9	+25.5	2.1	+23.8	1.0	+19.0	2.1	+20.9
—16.3	+13.5	1.1	+19.0	3.0	+20.1	1.2	+16.5	2.0	+17.3
—13.6	+11.8	1.3	+15.8	2.8	+15.8	1.0	+14.8	2.2	+14.4
—10.7	+ 8.5	1.7	+10.8	3.1	+12.5	1.4	+11.5	2.4	+10.8
— 7.5	+ 5.8	1.2	+ 6.3	4.8	+ 8.3	1.8	+ 6.5	2.1	+ 6.7
— 3.9	+ 3.5	1.6	+ 1.3	3.0	+ 3.8	2.0	+ 4.0	1.9	+ 3.1
0	+ 2.3	1.6	— 2.3	3.2	— 1.0	1.5	— 0.5	1.1	— 1.5
+ 4.3	— 0.8	1.2	— 7.5	2.5	— 5.3	0.2	— 3.0	2.0	— 4.1
+ 9.0	— 4.0	0.9	—11.0	4.2	—10.5	1.7	— 7.0	2.2	— 8.9
+14.2	— 7.0	1.5	—15.3	3.3	—15.6	0.5	—12.5	3.0	—12.6
+19.9	— 9.8	1.4	—19.3	2.3	—20.5	1.0	—18.0	2.8	—16.9
+26.2	—12.3	1.5	—21.8	2.5	—25.0	1.1	—21.3	3.3	—20.1
+33.1	—15.0	2.1	—23.8	2.6	—28.2	2.6	—25.0	1.8	—23.0
+40.7	—16.0	1.6	—26.8	4.0	—32.6	3.8	—30.3	2.1	—26.4
+49.0	—22.8	1.8	—33.0	2.8	—37.4	4.5	—37.3	4.2	—32.6

C, number of millimeters by which the diameter in Set A differed from that in Set B.

D_I, D_{II}, D_{III}, D_{IV}, number of grams by which the estimated weight of the block in Set A differed from its true weight, for four observers, I, II, III, IV; averages of 20 experiments each.

AD, average for the four observers.

MV, mean variation; the mean variation for the series is obtained by dividing by 4.5

accuracy of the observer was obtained by employing the method of suspending the blocks which will be explained in the next series of experiments. This test was made after the twenty regular experiments. The observer seated himself behind a screen and, keeping his hand in a comfortable position on the other side of the screen, lifted the blocks by the handles in such a manner that he had no knowledge or intimation of the size of the bodies lifted. The mean

variation thus found for the whole set of blocks is slightly less than the mean variation recorded in the table for twenty trials on any single block.

The results of these experiments, showing the persistence of the illusion when the actual weight is not known, are given in Table II.

This table proves that the illusion is regular and persistent. It cannot be eliminated by practice as long as the actual weight is not known.

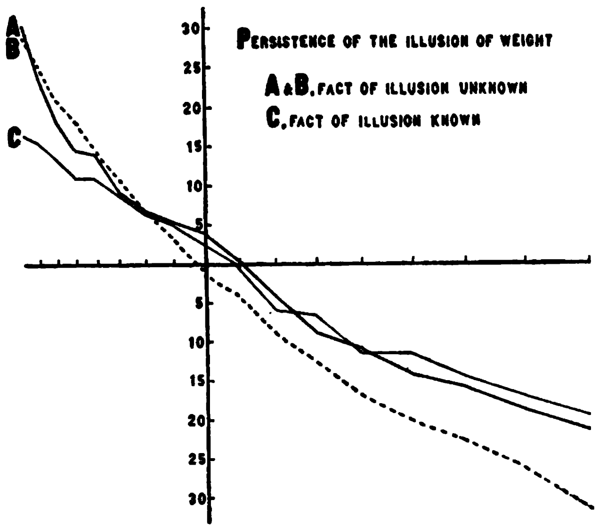

FIG. 3.

This persistence of the illusion justifies the procedure of making successive tests of the principle on the same observer under varied conditions for the purpose of comparison.

B. Actual relations of weight known.

Does the illusion persist when the observer is familiar with the apparatus and knows the nature of the illusion and the actual relations of weight? To test this, I used the same apparatus as before and the same method, except that the observer was made acquainted with every essential of the apparatus before beginning the experiment.

For observers I secured ten professors and graduate students who had all done special work in psychology and could be depended upon for reliable introspective analysis and critical judgment. Each

one knew the general trend of the illusion as found in previous measurements, but no one knew what the extent of the illusion was for other persons under the present conditions. They knew the actual weight and were to determine the apparent weight. This was very difficult because there was a conflict between knowledge and feeling, and the former was to be governed by a conscious effort. In previous experiments the observer was supposed to exercise all possible skill in matching the blocks correctly by lifting them, but

TABLE III.

Persistence of the illusion of weight when the fact of the illusion is known.

C	*AD*	*MV*
—22.9	+16.5	4.0
—20.9	+15.5	4.2
—18.7	+13.5	4.1
—16.3	+11.0	4.5
—13.6	+11.0	4.9
—10.7	+ 7.8	5.1
— 7.5	+ 6.5	3.4
— 3.9	+ 5.0	4.0
0	+ 2.3	4.4
+ 4.3	— 1.0	3.2
+ 9.0	— 6.0	3.0
+14.2	— 6.5	3.7
+19.9	—11.5	3.9
+26.2	—11.5	2.2
+33.1	—14.5	3.5
+40.7	—17.0	3.5
+49.0	—19.5	5.1

C, number of millimeters by which the diameter in Set A differed from that in Set B.

AD, number of grams by which the estimated weight of the block in Set A differed from its true weight; average for ten observers.

MV, mean variation; the mean variation for the series is obtained by dividing by 3.2.

here it was required to discriminate between known weight and felt weight and to estimate the amount of difference by matching the blocks as before.

The full record of the experiments upon these ten observers is contained in Table III.

The curves in fig. 3 give a diagrammatic representation of the intensity of the illusion (A,B) when the fact of the illusion is unknown, and (C) when it is known. A is the curve from Table I, fig. 2, placed

here for comparison; B, dotted line, is the average for the four observers, Table II; and C is the average for the ten observers, Table III.

The coincidence of the judgments of the observers is an extraordinary phenomenon, considering that the discrimination is very difficult under these circumstances, and that no one could be influenced by the records of others. The conclusions are plainly warranted that:

1. The illusion of weight persists even after the observer knows its nature, extent, and cause;

2. Its intensity is somewhat less than when the actual conditions are unknown to the observer.

To carry the test further and require the observer to repeat it a great number of times, would be to find out how long it would take him to learn to make the proper allowance for the illusory feeling. It is difficult to say whether that feeling can be wholly educated away, but we must suppose that a person can finally educate himself to make proper allowance for it or neglect it.

Third series of experiments: Dependence of the illusion of weight upon the directness of sight.

Thus far only the diameter in Set A had been varied. In the present series another quantity, the directness of sight, was varied. Each experiment consisted of four sets of tests. The aim was to secure a judgment of weight when the size of the bodies compared was made prominent in different degrees as follows: (1) size estimated by direct sight, (2) size estimated by indirect sight, (3) visual memory of size, and (4) no knowledge of size.

These conditions were obtained through the following four sets of variations, corresponding respectively to the above requirements:

1. The conditions of the first set were fulfilled by the method pursued in the previous experiments. The observer looked directly at the block as he grasped it between the thumb and second finger, and its proportions were emphasized by the comparative amount of space it occupied in the limited opening between the grasping fingers. The effort to grasp the disks accurately attracted the attention of his eyes in that direction.

2. In this series of experiments, additional apparatus was required for suspending the blocks. It consisted of handles, made of exactly the same length as the blocks and supplied with disks on the ends,

made of the same material and in the same shape as the disks on the blocks. To secure lightness they were made in the shape of a slender spool. These handles rested on a support in the same position as the tray, and from them the blocks were suspended 100^{mm} below, by means of silk cords. Each block was supplied with a loop of silk cord, which could readily be slipped off and on a hook at the lower extremity of the suspended cord. The combined weight

TABLE IV.

Illusion of weight under various circumstances.

C	*D*	*MV*	*E*	*MV*	*F*	*MV*	*G*	*MV*
—22.9	+24.5	8.9	+12.0	6.0	+ 8.5	4.4	—0.5	2.4
—20.9	+23.0	8.0	+10.0	5.0	+ 7.5	3.8	+1.5	3.7
—18.7	+19.5	9.0	+ 9.0	5.0	+ 6.5	3.9	—0.5	3.0
—16.3	+19.5	8.4	+ 6.5	4.5	+ 2.5	3.6	0	2.0
—13.6	+17.0	7.8	+ 4.0	5.0	+ 1.5	4.8	+0.5	1.5
—10.7	+12.0	9.3	+ 3.5	3.6	+ 2.5	4.6	—0.5	1.5
— 7.5	+ 9.0	6.0	+ 2.5	3.9	+ 3.5	5.0	—0.5	2.5
— 3.9	+ 4.5	2.5	+ 2.0	3.8	+ 1.0	3.6	—1.5	2.5
0	+ 1.5	2.8	— 1.0	4.0	+ 0.5	4.3	+0.5	2.5
+ 4.3	— 1.5	2.7	— 2.5	3.7	— 0.5	5.1	+1.0	1.0
+ 9.0	— 6.0	2.8	— 4.5	5.5	— 2.5	3.8	—1.0	1.6
+14.2	— 7.5	2.6	— 6.0	4.6	— 3.5	4.0	—1.0	1.7
+19.9	—14.0	3.2	— 7.0	4.9	— 4.5	3.8	—1.0	5.8
+26.2	—19.0	4.2	— 7.0	4.6	— 4.5	4.5	—1.5	4.6
+33.1	—18.5	3.5	— 9.5	5.4	— 5.5	1.5	—1.0	4.0
+40.7	—24.5	4.8	—10.5	7.0	— 8.0	2.6	+0.5	3.5
+49.0	—27.5	5.2	—12.5	8.2	—12.0	4.9	—2.0	3.4

C, number of millimeters by which the diameter in Set A differed from that in Set B.

D, *E*, *F*, *G*, number of grams by which the estimated weight of the block in Set A differed from its true weight; averages for ten observers in each series.

D, with direct vision of size.
E, with indirect vision of size.
F, with visual memory of size.
G, with no knowledge of size.
MV, mean variation; the mean variation for the series is obtained by dividing by 3.2.

of a handle with hook and cord was 2^{g}. Employing this method of suspending the blocks, the observer naturally directed his attention to the handle by which he lifted. Since the blocks hung 100^{mm} below the handle, they were out of the line of direct vision and not in contact with any body whose contiguity would limit the space occupied or contrast its size.

3. The weights were again suspended. The observer looked at each block as it was hooked on but shut his eyes while lifting it.

4. The observer was blindfolded for the final set. He was then required to lift the suspended blocks by the handles without knowing whether he was lifting a big one or a little one. He remembered the range of the sizes but did not know which block in the series was presented.

The purpose of the variations was not told to the observer. He was simply asked to subject himself to four tests necessarily to be taken successively on the same observer. Definite instructions were

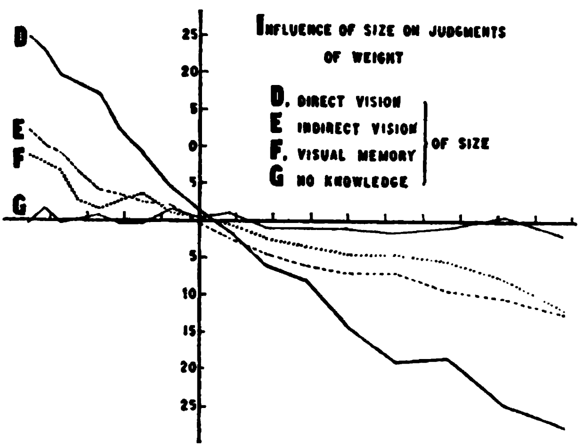

FIG. 4.

given at each step so far as this could be done without making any suggestions pertaining to the illusion. The whole experiment on one observer occupied from 80 to 100 minutes, sufficient rest being allowed between the different sets. The observers were especially cautioned not to let the results of one test influence their judgment in another.

The following statements are based upon the results contained in Table IV (fig. 4).

1. Visual knowledge of size causes the greatest illusion, in determination of weight, when the body lifted is directly looked at and its size is brought into prominence by the comparative amount of space that it occupies in a limited area.

2. The influence of size on judgment of weight is lessened when the object is placed out of the line of direct vision and isolated from any object whose contiguity would favor a comparison of size.

3. Visual memory of size causes a less intense illusion of weight than that which is produced by looking at the blocks while lifting.

4. Size has no influence on the perception of weight when the observer has no knowledge of it.

Fourth series of experiments: Dependence of the illusion of weight upon the senses by which knowledge of size is acquired.

In all the foregoing experiments the perception of difference in size was purely visual. The next problem was to settle the question: Does the illusion of weight vary with the different senses by which the image of size is produced?

It is impossible to draw a strict line of demarcation between the different sensory elements which, besides sight, unite in building up a percept of size, because they generally coöperate, and their effects fuse. For the purpose of comparison, I aimed to get a measure of the illusion of weight due to size when knowledge of size was acquired through each of the following channels predominatingly: (1) muscle sense, (2) touch, (3) sight, (4) muscle sense, touch and sight.

These conditions were approximately attained by the following respective methods of procedure.

1. Without having seen the blocks, the observer seated himself on a high stool behind a screen, in such a position as was occupied when he stood by the tray and looked at the apparatus, so that his arm and hand would be comfortably adjusted over the tray, on the other side of the screen. The blocks were then handed him in the same order as before, but placed on end on the tray, so that by dropping his thumb and fingers around a block the observer included it in his grasp, holding it by the circumference; he thus acquired a knowledge of its size mainly by the muscle sense in the fingers.

2. Seated in the same position, after a brief rest, the observer held out his hand, not resting it on anything, and the blocks were placed on his flat palm, one at a time. An image of the height (length) of the block was inevitably transmitted from the experience in the first set, but the proportional size was here estimated chiefly by the area of touch or pressure.

3. The method of estimating comparative size by direct sight, as pursued in the previous experiments, eliminates all other sensory elements than sight.

4. In the final set the blocks were again placed on end, and the observer occupying the same position as in the third set, grasped the blocks by the circumference as in the first set of this series and, in addition, looked directly at them. Hence, there was the combined effect of muscle sense, touch and sight.

Upon the basis of the results of the second series of experiments, I considered it justifiable to make these four successive tests on the same person. Furthermore the advantage of the order will be noticed. In the first and second sets he had not seen the blocks. The estimate of the length was a proper influence to carry over to the second set ; it was, namely, a memory of the fact that the length was a constant quantity. The third set could not be influenced by any previous judgments, because the record was kept secret, and the blocks had not been recognized. In the fourth set a warning against influence from previous judgments was really superfluous, because the observer was after truth and not consistency.

No test on the error of observation, or mean variation when there was no influence of sight, was here included, because that would practically be the same as found in the fourth set of the third series, and the record of A (9) in each set.

The sensations of touch and pressure cannot be distinctly separated. Indeed, since the blocks cannot be placed without some velocity, there is really a sense of impact in the second set. For brevity, the space-giving characters of these sensations is spoken of as "touch."

That muscle sense, touch, sight and other sensations cannot be isolated does not detract any from the value of this series of experiments. In the first set, skin and joint sensations assist; and in the second set, joint and muscle senses are slightly involved.

In the third set, all of these senses join in determining the size of a body, but since all but one are constant, it remains for that sense, sight, to discriminate for differences in size.

In the first set, the observer had not seen the blocks. The experience that they differed in size puzzled him. Hence every time he picked one up his first concern was to fix an image of its size by estimating the relative extent of the grasp required in lifting it. In the second set, attention was again called to the varying size in a similar manner, but here the estimate of the dimensions was more indefinite and uncertain. In the third set, he determined the dimension at a glance, without any effort, and hence did not concern himself particularly about the estimation of size. But, in the final set, his attention was again called to the appreciation of size, because

here he noticed that he combined several sensory elements, in that way acquiring a very accurate knowledge of size.

In brief, by the above four variations, different degrees of attention were given to discrimination of size, while to avoid the illusion all attention should have been given to the perception of weight.

According to this method a constant error, due to the fact that A was lifted first, will be implied in the first two sets. I could not

TABLE V.

Illusion of weight for different senses.

C	*H*	*MV*	*I*	*MV*	*J*	*MV*	*D*	*MV*
—22.9	+27.5	10.0	+12.5	16.5	+21.5	9.0	+20.5	9.1
—20.9	+20.0	12.2	+15.0	13.0	+16.0	8.8	+20.5	8.8
—18.7	+21.5	13.0	+10.0	12.5	+17.0	8.6	+19.5	8.5
—16.3	+12.5	9.4	+ 6.0	11.7	+12.5	9.5	+17.5	5.4
—13.6	+ 9.0	6.6	+ 1.0	12.5	+ 7.0	7.6	+18.5	7.4
—10.7	+ 4.0	9.0	+ 5.5	9.4	+ 8.0	8.0	+10.0	6.0
— 7.5	+ 2.0	6.2	+ 0.5	8.8	+ 3.5	7.4	+11.0	7.2
— 8.9	0	3.0	— 1.0	7.1	+ 5.0	6.5	+ 4.5	6.5
0	— 7.0	6.2	— 5.0	5.5	— 1.0	8.1	— 1.0	6.8
+ 4.3	—12.0	5.5	— 9.5	6.3	— 2.5	9.8	— 1.5	7.2
+ 9.0	—18.5	7.0	—14.0	7.8	— 5.5	7.5	— 4.0	9.3
+14.2	—22.0	8.4	—18.0	6.0	— 7.5	5.8	— 9.5	6.5
+19.9	—26.5	7.5	—19.5	7.5	— 9.0	6.8	—14.5	4.9
+26.2	—28.5	6.8	—24.5	8.2	—15.0	9.0	—15.5	5.2
+33.1	—35.0	5.0	—33.5	7.2	—13.0	7.2	—22.5	6.6
+40.7	—34.0	4.4	—31.0	8.7	—18.0	8.1	—25.5	7.5
+49.0	—38.0	4.9	—33.0	11.0	—20.0	8.0	—28.0	8.0

C, number of millimeters by which the diameter in Set A differed from that in Set B.

H, *I*, *J*, *D*, number of grams by which the estimated weight of the block in Set A differed from its true weight; averages for ten persons.

H, size known by muscle sense.

I, size known by touch.

J, size known by direct sight.

D, size known by muscle sense, touch and sight.

MV, mean variation; the mean variation for the series is obtained by dividing by 3.2.

eliminate it by repeating the test in the reverse order, because it would be too fatiguing to take each set twice. It was more important to take the four sets in regular succession than to divide up the experiment in order to eliminate the error. Hence, the fact of this constant error must be borne in mind in interpretation of the results. An approximate correction of it may be made by noticing its extent in the middle block of Set A.

The results are given in Table V and are expressed in the curves of fig. 5.

The variation of the intensity of the illusion of weight, due to the acquisition of knowledge of size by different senses, may be formulated on a comparison of the above four sets of results as follows:

1. The illusion of weight dependent on size is greatest when size is estimated mainly by muscle sense, and the weights have not previously been seen.
2. The illusion is more fluctuating and on the whole not quite so strong when size is estimated by the area of pressure in the flat palm, including a memory of the third dimension.

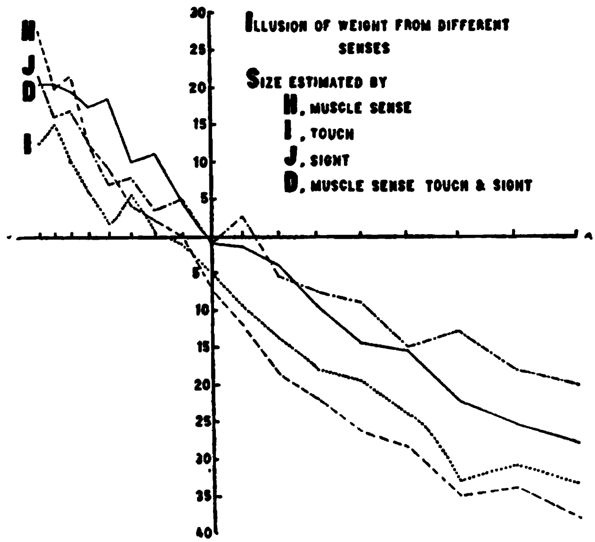

FIG. 5.

3. In these variations the illusion is weakest when size is estimated by direct sight.
4. When size is estimated by the combined effect of all the spatial senses, the illusion is weaker than when depending on muscle sense or touch and stronger than when dependent on sight alone.

Fifth series of experiments: Illusion of weight due to the knowledge of the material of which the weights are made.

This series of experiments pertains to a corollary of the foregoing conclusions. Is there an illusion of weight dependent upon disappointed expectation caused by a supposed knowledge of the material

lifted? Theoretically, if size has a suggestive influence upon judgment of weight by leading the observer to expect a body to be heavy or light according as it is large or small, a similar association between judgment of weight and knowledge of the specific gravity of the material lifted ought to cause an illusion of weight, under favorable circumstances. If a body, seen to be made of a heavy material but containing a light deceptive core, be lifted, it will rise too easily because lifted with an effort calculated to raise a heavier weight, and it ought to be underestimated for the same reason that a large block in Set A is underestimated. In the same manner, if a body apparently made of a solid piece of light material but containing a heavy deceptive core be lifted with a motor adjustment adapted to the apparent weight, it will require more effort than was expected to raise it, and it ought to be overestimated for the same reason that the small block in Set A is overestimated.

This problem suggests a wide range of experiments, but there are very great difficulties in the way of an accurate measurement. The methods I have employed in this series are only partially successful. An attempt was made by three sets of experiments to bring out the following variations in the conditions of the bodies to be lifted:

1. Same size and weight but different material;
2. Same size but different material and weight;
3. Different size, weight and material.

First set.

Four blocks were made of the same shape and size as the B's, i. e. 42.9^{mm} in diameter, and the same weight as the A's, i. e. 80^{g}. One was made of cork, one of wood, one of copper and one of lead. The first two were filled and the last two were cored out until each one weighed 80^{g}. This was done so carefully that the surfaces in their natural color suggested that the weights were turned out of solid pieces of the respective materials and were uniform throughout. It was thought that the observers would possess a fairly definite knowledge of the specific gravity of these four kinds of material.

The method was to require the observer to take one of these at a time and match it in the original Set B. Ten observers tried the experiment, but the amount of error which may be due to the illusory influence is not more than what must be allowed for the mean variation. This set was a failure because the lead was too light to allow any one to suppose that it was solid, and the cork was so heavy that

the observer at once concluded that it was filled. The illusion was quite effective in the wood and the copper blocks, but the judgments of these were influenced by the deceptive appearance of the other two, so that the mean variation was large.

Second set.

Three blocks were made the same size as the B's, i. e. 42.9^{mm} in diameter, but of the following materials with their respective weights: (1) cork, natural weight 40^{g}, loaded to 55^{g}; (2) wood, natural weight 65^{g}, loaded to 80^{g}; (3) lead, natural weight not determined, cored out so as to weigh 120^{g}. Their appearance suggested that they were uniform throughout. The aim was to make them feel different from what the observer expected them to feel; but not so much that he would suspect that they had a false appearance. Nothing was said by explanation or question as to the character of the material before beginning the experiments. After these experiments about 60% of the observers stated that they had taken the blocks to be made solid of the materials seen on the surface. About 20% were uncertain, and the rest thought that they had a deceptive appearance.

Fourteen observers gave this result in selecting corresponding B's for the lead block: one overestimated, six right, and seven underestimated. The average amount of the underestimation was 8^{g}.

In the same manner fourteen observers gave the following results in matching the cork block: one underestimated, four right, and nine overestimated. The average amount of the overestimation was 7^{g}, which may be considered as due to the 15^{g} surreptitious weight.

The twenty-nine observers who matched the wooden block gave these results: three underestimated, seven right, and nineteen overestimated. The average amount of overestimation was 10^{g}.

The apparatus cannot be counted a real success until it contains an appreciable illusion for all observers and still has no suggestion of deceptive appearance. Nevertheless this fragmentary set of experiments shows that:

1. When a body containing a surreptitious light core is lifted with a motor adjustment adapted to the expected weight of a certain material and it feels lighter than was expected, that body will be underestimated in weight; and

2. When the body lifted contains a deceptive heavy core, the observer will overestimate the weight of that body, especially if he does not suspect the presence of the core.

Third set.

The largest and the smallest of the blocks of Set A were used and in addition two extra blocks were made, here called respectively P and L for brevity. P was a solid piece of pine wood turned to exactly the size of the largest A, which made it weigh 75^g. L was a solid piece of lead turned in the shape of the smallest A until it weighed 75^g; the length was kept the same as that of A, but the diameter had to be reduced to 16^{mm} in order to secure the proper weight. This made it a little more than two steps smaller than the smallest A, according to the ratio of the diameter of the A's.

The aim was to determine whether the illusion would be greater between the largest and the smallest blocks in A, which appeared to be made of the same material, than between P and L, whose apparent material suggested the proper weight. By the method followed each of these four blocks was to be matched twice in Set B, taking them in reverse order so as to avoid errors due to the order of sequence. Eighteen observers had two trials each. The averages of the B's selected were: For largest A, 4.6; for smallest A, 13.4; for L, 12.8; and for P, 5.3; with the corresponding average mean variations: For largest A, 6^g; for smallest A, 9^g; for L, 10^g; and for P, 7^g. Hence, although P weighed 5^g less than largest A, it was estimated to be on the average 3.5^g heavier, making the illusion in P 8^g less than the illusion in largest A. And, although L was over two steps smaller than smallest A, and weighed 5^g less, it was judged to weigh only three grams less. Taking the average of the results previously obtained for the A's, the illusion for L is found to be about 7^g less than for smallest A.

In the thirty-six trials the illusion was stronger for largest A than for P twenty-nine times, equal six times and weaker once. Similarly it was stronger for smallest A than for L thirteen times, equal nine times, and weaker only fourteen times, although L was over two steps smaller than smallest A.

I place no great confidence in the quantitative determinations in this series of experiments, because not all the conditions were sufficiently controlled, but the general qualitative results are positive. It has been proved in two different ways that knowledge of the material lifted influences the judgment of weight, namely:

1. When there is no illusion on account of size, there is under certain circumstances an illusion caused by knowledge of the material lifted; and,

2. When there is an illusion due to size, it is not so strong when the difference in size is proportional to the specific gravity of the material lifted as when there is no definite knowledge of the material.

General remarks on the experiments in Part First.

Owing to the nature of the subject the method employed is necessarily a combination of the experimental and the statistical. It is experimental because, whenever practicable, I have varied the conditions and repeated the tests on the same individual. On the other hand, a sufficient number of persons from the various vocations have been tried to satisfy the statistical demands and justify the conclusion that the main laws derived are fairly universal.

Apparatus.

The difference between the B's was made 5^g, because in an arithmetical series that is about the average limit of discrimination for weight within the range here investigated. The possible variable error due to this cause is $\pm 2.5^g$. This source of error is evenly distributed throughout the series; hence, it does not alter the expression for the general intensity of the illusion, but merely causes minute fluctuations in the curves of the results.

If a wider range had been investigated, the B's should have been made to vary in weight by an increment of about $\frac{1}{17}$ according to Weber's law, in order that the perceptions of difference should have been equally appreciable in discriminative consciousness. Here a constant difference was used merely for convenience, but that arrangement ought not to modify the results appreciably.

The additional 2^g of the suspending apparatus is the same for all the blocks, and this increase in the total range of the weight has so small effect on the results that it may practically be neglected.

In a series of cylinders of equal length but varying diameter, there is a constant illusion in regard to length. The apparent length increases with the decrease of the diameter. In order that these blocks should have appeared to be of the same length they should actually have varied in geometric ratio with a very small increment. This could not be done in the present test because one of the required conditions was that the grip should be uniform.

The temperature of the blocks was kept practically constant.

Observers.

The following essential requisites were kept in view in selecting observers:

1. They shall be careful and competent observers;
2. They shall have no previous knowledge of the special principles involved or of the direct purpose of the experiment;
3. They shall not know how or of what material the blocks are constructed;
4. They shall be in a comfortable condition and free from emotional disturbances;
5. They shall be trusted not to work according to any prejudicial scheme or preconceived theory.

These qualifications could only be secured in a relative manner, but the aim was scrupulously adhered to so far as resources and temporary conditions permitted. Discrepancy in the attainment of these requirements accounts for many of the partial variations from the general law of the illusions. The requirements were altered for the fifth and the latter part of the second series so as to suit the special demands of those cases. On the whole, I was fortunate in securing the assistance of reliable observers, who worked with a scientific interest.

Elimination of errors in method.

There is a law that when two bodies of approximately the same weight and size are lifted in succession, the weight of the latter is overestimated. The extent of this error varies considerably for different persons and for the same person at different times. In the present experiments it was, however, practically eliminated by requiring the observer to lift two blocks, to be compared alternately several times before forming his decision.

In certain cases there may have been a slight error due to the order in which the blocks were taken up to be matched. This applies to both the A's and the B's. Thus in the second series it was found that the curve of the illusion is a little more regular when the A's are taken in order than when they are taken at random. In the same manner the illusion might depend slightly on how many B's were picked up and tried before a decision was reached. These influences may have affected some single choices, but they do not appreciably change the expression for the general intensity of the illusion. This was proved by actual measurements.

The memory image for weight sinks very rapidly during the first ten seconds. The blocks were therefore placed so that the observer could change from one to the other by the least possible movement and change of position. He could deliberately lay down one and pick up another in three seconds. The time required for this transition was the same for all the blocks.

There is an illusion of weight caused by fatigue in lifting objects. This illusion did not affect the present experiments because the bodies compared were lifted alternately several times. Furthermore the number of trials was always limited to such an extent that there would be no necessary cause for any disturbing fatigue from continuous exertion.

The known error in judgment of weight caused by lifting a light body after a heavy one, was here avoided by the fact that the final discrimination was not made until the observer had reached two blocks that appeared to weigh the same. The error was frequently involved in the first trials of any experiment where there was a decided difference between the blocks, but as the observer found his way up to those that appeared more and more alike, it was gradually eliminated before he reached a final decision.

The weight of a body depends upon its mass and the velocity with which it is lifted. Therefore special precaution was always taken to get the observer to lift the block to the same height and with the same velocity as nearly as possible.

In the case of direct sight the observer would often for a moment try to avoid the distraction of sight by not looking at the block. Such judgment would then inevitably be exceptional and be governed by the conditions of indirect sight or visual memory.

Some partial variations from the general trend of the illusion in direct sight are due to this cause. When the observer took such liberty only once or twice, I did not interfere, but if he proposed to continue I was obliged to inform him, in some guarded statement, about the conditions required.

Suggestions for further experiments.

I chose the diameter of middle A as the diameter for all the B's, merely because it was the middle. The B's might have been made any size within the range of the A's. As a development and further proof of the present test, the following variations might be tried.

1. Construct the B's all the same size as the smallest A, making one 90^g and the rest decreasing by steps of 5^g each, down to 25^g. That would allow the same range of the illusion as the present apparatus, since the smallest A might be judged fully three times as heavy as the largest A.

2. Construct the B's all the same size as the largest A, making the lightest 70^g and the rest increasing by 10^g each step up to about 240^g.

3. Similarly construct the B's in separate sets respectively of the same size as each of the intermediate A's.

From tentative tests made it appears that the illusion would have approximately the same intensity under these conditions as was determined by the apparatus and method employed. The difference between the principle of the first two classes of apparatus suggested and the one employed is that, according to the former, a quantity is measured by a direct comparison which, according to the latter, is measured by an intermediate step. This would be true when two A's are compared by means of the B's. When an A is compared with a B, the process is the same in both cases. It will be observed that in all these suggested experiments it is the same objects that are measured, but the measuring is done by different means in each set of experiments.

How does the illusion vary with a greater variation in diameter of the cylinders, with different ranges of weight, and with variation in the shape of the weights? These and similar questions demand solution by experiment, but the measurements can not be made as definite and as accurate as those in the present research.

Dr. Scripture has a set of weights made large enough to use in demonstrations before large audiences. It consists of cubical wooden boxes loaded with lead to the desired weights. There are only two corresponding to my A's, the smaller 8.2^{cm} cube and the larger 60^{cm} cube, each weighing 8¾ lbs. Those that correspond to my B's are all 15^{cm} cube and range by ¼ lb., each step from 2 lbs. up to 17 lbs.

I tried the experiment with this apparatus on four men with the following result: The large box was matched with boxes of 5.75 lbs., 4.25 lbs., 4.25 lbs., and 2 lbs., by the respective observers—average 4 lbs.—that is there was an underestimation of 3 lbs., 4½ lbs., 4.25 lbs., and 6.75 lbs. respectively—average 4.4 lbs. The small box was matched with 8.5 lbs., 14.75 lbs., 13.25 lbs., and 10.25 lbs. respectively—average 11.9 lbs.—that is, there was a corresponding overestimation of 0.5 lb., 6 lbs., 4.25 lbs., and 1.5 lbs.—average 2.9

lbs. That means that mainly on account of this difference in size the observers made a difference of 2.75 lbs., 10.5 lbs., 9 lbs., and 8.25 lbs. respectively—average 7.6 lbs.—between the two boxes, which both had the same weight, 8.75 lbs. That is, the smaller is judged to weigh 2.9 times as much as the larger. This, it will be observed, slightly exceeds the average amount of the illusion between the extremes in my set.

This illusion is based upon the difference in volume, but a comparison between Dr. Scripture's set and mine shows that it depends very much upon the shape of the weights and whether two or three dimensions are varied. The diameters of the smallest and the largest A are in the ratio of 20 : 91.9 ; while the one dimension of the boxes stands in the ratio 8.2 : 60, and still the illusion is not much stronger in the latter case. It is therefore evident that it does not vary directly with the volume. The range of the weight is the great factor which must here be taken into consideration. It would be very desirable to try the following variations :

1. Extend Set A with additional blocks on both ends, making the smallest as small as possible when a shell is filled with mercury to 80^g and the largest as large as possible without exceeding the given weight. Handle them in the most convenient way and match them in an extended series of the B's to determine whether the illusion varies with the variation in diameter according to Weber's law.

2. With the same scale of variations in size make different sets of different weight. Sets of 40^g, 160^g, and 320^g would perhaps be the best weights to try in addition to the original set of 80^g.

3. Since the illusion depends very much upon the shape of the body lifted, it would be very interesting to determine the comparative amount of the illusion when all three dimensions are varied as in regular cubes or spheres.

DRESSLAR[1] found that of bodies (lead sheets) which have the same weight and the same size but different shape, the one which appears the most compact, the circle, will be judged to be the heaviest. The same principle ought to apply to the cube and the sphere ; and it actually appears that this is confirmed by Dr. Scripture's cubical boxes as well as by CHARPENTIER'S[2] brass balls.

Since on impact the weight of a body varies with its mass and the square of the distance through which it falls, the normal association

[1] DRESSLAR. *Studies in the psychology of touch*, Am. Jour. Psych., 1894 IV 49.

[2] CHARPENTIER, *Analyse de quelques éléments de la sensation de poids*, Archives de Physiol, 1891 (5) III 126.

between weight and velocity of impact is quite strong and definite. Hence on the basis of what has been determined the following experiment may be suggested: Construct an apparatus such that a ball may appear to fall through a given distance and impinge upon the hand; but, by a secret device, it falls only a part of that distance or conversely, through a greater distance. The disappointed expectation in regard to the velocity of impact ought to cause an illusion which might readily be measured.

The difficulties encountered in the fifth series of experiments urge the investigation of another problem, the effect of color on discrimination of weight. Several observers stated that it had a decided influence on their judgments. The experiment might be performed somewhat as follows: Make a standard set of twenty blocks corresponding to the B's above, of an "indifferent" color. Let these vary by steps of 3^g each, making the ninth weigh 80^g. Make another set of eighteen blocks, nine of the "indifferent" color and the remaining nine of each of the respective spectral colors, together with black and white. Let these last nine be weighted to 80^g; four of the "indifferent" to 77^g, 74^g, 71^g, and 68^g each; and the remaining five "indifferent" to 83^g, 86^g, 89^g, 92^g, and 95^g each. Require the observers to match each block from the second set with one in the first set, which will serve as a measure.

Dresslar[1] seems to make an unjustifiable inference in regard to the variation of this illusion with the degree of intelligence of the observer. He used eight cylinders having a constant weight 132^g, a constant diameter 1 inch, and length varying by steps of one-half inch each. Such a difference causes quite a decided illusion for everybody. He concludes that those who arranged them in regular order according to the illusion were the most suggestible. That is not necessarily so. All that he can conclude is that those who arrange them in regular order have the finest power of discrimination; for since the illusion exists for all persons, an error in the arrangement is the same as an error of observation when there is an absolute difference in weight. One who has a very fine power of discrimination may arrange the weights in perfect order according to the illusion even if the illusion is not half as strong with him as with another person who is a poor observer and cannot judge weights well enough to arrange these in order. Dresslar's apparatus does not afford him any definite measure for the intensity of the illusion.

[1] Dresslar, *Studies in the psychology of touch*, Am. Jour. Psych., 1894 IV 41.

To justify his conclusion he should have measured the extent of the illusion for the different classes of observers. This GILBERT[1] did in his tests on school children, and he found that the intensity of the illusion decreases quite regularly in children from the age of 9 to 17. From 6 to 9 the suggestion increased, but from 9 to 17 inclusive it decreased, the amount of the illusion for his apparatus being as follows for the respective ages beginning with 9 : 50.0^g, 43.5^g, 40.0^g, 40.5^g, 38.0^g, 34.5^g, 35.0^g, 34.5^g, and 27.0^g. If we admit that children at 17 have a higher degree of intelligence than children at 9, this proves just the reverse of DRESSLAR's conclusion. To experiment further upon this very important problem, a very satisfactory arrangement would be had by taking DRESSLAR's method of classifying pupils into three classes according to general intelligence and testing them by the apparatus used in this research.

Psychological analysis.

Perception of weight is an interpretation by discriminative consciousness of a very complex series of peripheral sensory elements with a conative feeling of effort in the light of an intricate series of associated ideas. An illusion of weight is caused when any of these factors stand out in an unnatural relation. Thus if the skin be inflamed, the haptic sensations will be abnormal ; if the muscles be fatigued or the joints disordered, the muscle sense and the joint sense will give a wrong impression ; if the central associations through memory, imagination, and comparison be mistaken, the interpretation will be false ; and if the exerted effort be disproportionate to the object, the expended energy will be incorrectly estimated.

In order to see the psychological process in the present illusion, let us take an example, magnify that instantaneous perceptive process, and analyze it into its constituent elements in a schematic way.

The weight of an object is to be determined by lifting it in view. The first step consists in the presentation of a sensation complex, which is the sensory report of the eye and hand upon seeing and grasping the object to determine its spatial relations. Though knowledge of size may not be necessary for the percep-

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[1] GILBERT, *Researches on the mental and physical development of school children*, Stud. Yale Psych. Lab., 1894 II 60.

tion of weight, it always enters as a preliminary determination. I cannot conceive what kind of a sense of weight a person would have without some estimate of the volume through which it is distributed. The presentations of the spatial sensations first fix in discriminative consciousness an image of the size of the object, and this is compared with the memory images of experiences with similar objects. In a comparison of the surface appearances in color, shape, nature of material, smoothness, hardness, etc., it is the function of attention to bring all these considerations into service to determine the probable weight on the basis of previous experiences.

After an elaborate and infinitely complex process of discrimination and comparison, the result is that a probable weight, w, is assigned to the object. This whole mental process was performed before the object was raised from its base. The motor apparatus had waited for a command from discriminative consciousness, directing what amount of effort should be put forth in order that the motor adjustment should correspond to the mass of the object. In consideration of the estimated probable weight, w, the gauge of the motor impulse is set to a corresponding amount of motor effort, w. The judgment of weight is an interpretation and estimation of the amount of this effort, w, expended.

If the effort put forth just meets the demand, the weight will be judged accurately, other things being equal. But if some greater effort, $w+d$, should be required in succeeding to lift the object, the extra effort, d, is brought into exceptional relation to w, and this causes d to become prominent and overestimated. Since d is an increment of w, $w+d$ will be overestimated, i. e. the weight of the object will be overestimated. And, if $w-d$ should be required, d will again be overestimated for the same reason. Since d must be subtracted from w, $w-d$ will be underestimated, i. e. the weight of the object will be underestimated.

Here we have taken for granted that w was determined by our general past associations between weight and size. But, suppose in a particular case, largest A and smallest A are to be lifted in succession. Since they have the same appearance except in size, there is a special and very definite association formed between size and weight, which raises the value of d by contrast and makes it more definite. The greater the value of d, the greater will be the consequent illusion. Hence, this illusion of weight

depends upon our more or less fixed associations between size and weight.

Sight first scans the object, estimating its size; then come the skin, joint, and muscle sensations and emphasize its reality and solidity. When this is done, active motor consciousness proceeds to estimate its weight. Or, to emphasize the different steps again, the size of the object is first estimated; then, on the basis of that and the appearance of the material, a mental estimate of its probable weight is formed; and, with the muscles accommodated to this, the actual weight is judged by estimating the amount of expended effort.

The above analysis corresponds to the actual process of perception in the case of the illusion under consideration. Let us trace its process more particularly in different stages.

In a preliminary way two objects, largest A and smallest A in the above apparatus, are to be compared in weight. The observer looks at both and then lifts largest A, noticing its size particularly. It is found lighter than was expected. Consequently a certain amount of effort, *d*, must be subtracted from *w*. Therefore there is an illusion: largest A is underestimated. Then he lifts smallest A in the same manner. Since the two objects appear to be made of the same uniform material, and the weight of largest A is known, the probable weight of smallest A is estimated to be in proportion to the size of the objects; hence the intensity of *w* is based upon a comparison of the diameters of the two objects. Consequently *d* is raised to a very high value by the disappointed expectation due to contrast, and smallest A is very much overestimated. In both the overestimation and the underestimation there is evidently a surprise or disappointed expectation.

This explanation is applicable in the first trial, where there is a surprise. But how explain the persistence of the illusion in repeated trials with the same block as well as with a series of blocks?

Suppose the observer has matched the blocks 19 times and come to a fairly positive conclusion that the smallest one is twice as heavy as the largest one. Then he knows just what to expect the 20th trial, and there ought not to be any surprise. But the illusion is there just as before. Could we neglect or break up a series of fixed associations that have been forming in all our past life, then there ought theoretically not to be any extensive persistence of the illusion. But we cannot. The judging activity is not in such perfect control that all the information gained by the first trial can

be utilized and all previous associations set aside. Instead of taking a new course in the second trial and estimating the amount of effort only by the experience with these particular blocks, interpretative consciousness ignores that and follows the formerly set paths of habit. It receives the new reports from the spatial senses for each trial, compares, discriminates, and commands as before, often irrespectively of the immediately preceding experience. This is liable to continue as long as the observer is not informed of the illusion.

But how explain the persistence of the illusion when the observer knows all about it? Suppose he has been told that two objects have the same weight, and he has empirically discovered that the weight seems to vary inversely with the size. He lifts them and the original illusion persists. The same explanation must here be extended. He does not possess will power enough to disregard the accustomed manner of association and put into practice what he ought to know. Size has ever before been influential in determining weight, therefore, relatively, it can not be suppressed. This is not a sign of weakness in discrimination or judgment; it is the working principle for those whom we consider most intelligent. That feeling of interest which sight commands is persistent and insists on distributing the felt weight throughout the perceived volume; and in the ordinary flow of conscious activity, it is almost impossible to muster force enough to dam it up.

The same principle is confirmed by the variation of the illusion with the directness of sight. It is illustrated by the four steps in the third series of experiments. If we may represent attention as a force, attention to spatial sensations and attention to direct sensations of weight are two forces which have generally worked in the same direction; but, under these illusory circumstances, the former has turned against and opposes the latter. As one decreases the other increases. In direct sight attention to size has a strong opposing force to attention to weight; in direct sight the former decreases and the latter increases; in visual memory the corresponding increase and decrease is still greater; and, when there is no knowledge of comparative size, attention to size loses all force and there is no illusion.

The same principle is again exemplified in the fourth series of experiments, though somewhat differently. Previous to this the knowledge of comparative size had been acquired by sight. Here sight was excluded in the first two steps. Size was estimated by the other spatial senses. It is more of an effort to determine size by

touch or muscle sense than by sight. Hence it requires more attention, which would otherwise be accorded to the sense of weight.

Again it is evident that the same principle suffices to explain the illusion due to the suggestive force of the material.

In a word, then, the illusory influence of size upon judgment of weight consists in disappointed expectant attention. We continue to be disappointed because we will not take the temporary circumstances into sufficient account.

PART SECOND.

Experimental applications of the principle of suggestion in normal presentations of sense.

There is no dearth of observations on "errors of sense," nor need we review much of our normal experience in order to discover the presence of the force of suggestion. I have not undertaken to search particularly for new classes of illusions and hallucinations. The purpose in the following experiments has rather been to take up some of the simplest forms and principles of illusions and hallucinations and submit them to examination by applying the laboratory methods of psychological experiment to normal waking life.

The chaotic and superstitious view of mind represents it as lawless and unknowable, while on the other hand we have the view that by scientific method and philosophical introspection the laws of mind—mind, normal and abnormal—may be studied and classified just as well as the laws of matter. Again, on the one hand we have the phenomena of hypnotism, mind reading, crystal vision, apparitions, telepathy, etc.—considered as occult practices, while on the other hand there is an attempt to bring them in conformity with principles at work in normal every-day life. And yet again we have the claim that the world of things is but an illusory creation of imagination, while on the other hand there are those who regard the states and actions of mind as fleeting phantasms and place implicit faith in the presentations of sense.

Any attempt to discover the nature of sensory illusions and hallucinations deals with the core about which these contentions center. It is surprising that so little real scientific work should have been done in a field so important. The whole subject of "Trugwahrnehmungen" readily presents itself for experiment and it may

well profit by the recent developments in laboratory methods and facilities for experimental psychology. Working in this direction, I have the disadvantage of being one of the first to enter this domain, and consequently the method is tentative and cursory.

Hallucinations of warmth.

Apparatus.

The apparatus[1] for this experiment was constructed on the principle that a wire will be heated by passing an electric current through it. Two binding posts were placed 8^{cm} apart and connected by a German silver wire. They were then connected in circuit with a bichromate dip cell and a secret open-circuit key. The battery and the stand supporting the German silver wire were placed together on a table in front of the observer, while the open-circuit key was fastened under the table, where it could be readily opened and closed with the knee without being detected.

Method of experimenting and results.

The experimenter and the person experimented upon were seated, one on each side of the apparatus, facing each other. The preparatory explanation had to vary with the observer's knowledge of the principles involved. His attention was called to the fact that an electric current will heat a wire and that it will take an appreciable time for a person touching the wire to perceive this heat. The instructions were: "At a signal which I will give, after the dip is lowered, grasp that wire between your thumb and index finger; concentrate your attention on what you are doing, and when you feel the wire become heated, say *hot*."

All being ready, I lowered the dip as mechanically as possible and a second later gave the signal to grasp the wire. The time between the signal and the reaction was recorded in seconds. In the first two to five trials I closed the circuit with the knee key, the wire was heated, and we found roughly the time that it took to perceive the heat. The observer thus received a distinct perception of how the wire became heated, associated this with the lowering of the dip, and formed a distinct estimation of the required time.

Then I suggested that we repeat the experiment a sufficient number of times to secure a fair average. We proceeded exactly as

[1] SCRIPTURE, *Tests on school-children*, Educ. Rev., 1893 V 61.

before, except that I left the current broken; thus, although I lowered the dip, no current went through the wire.

Five different signals were used, namely: (1) a wire coil whirled so as to rattle and expose a red card, having an effect on eye and ear (the starting of this whirl was the signal to grasp the wire); (2) tapping the right hand with a pencil (same hand as held the heated wire); (3) tapping the left hand with a pencil; (4) tapping on the table; (5) saying "Now;" (6) tapping the wrist.

The experiments were made in sets of ten trials, using a different signal for each set. The signals were taken in the order given above. When no heat was perceived in one minute, that experiment was recorded as a failure. The results are contained in Table VI.

TABLE VI.

Hallucination of warmth.

O	*Signal 1.*			*Signal 2.*			*Signal 3.*			*Signal 4.*			*Signal 5.*			*Signal 6.*			*Summary.*			
	T	MV	F	T	MV	F	T	MV	F	T	MV	F	T	MV	F	T	MV	F	M	N	E	A
I	10	2	0	13	6	0	17	10	0	9	3	2	--	--	--	--	--	--	40	38	2	12
II	14	5	0	16	4	0	21	3	0	19	3	0	19	4	0	--	--	--	50	50	0	18
III	27	7	2	26	4	0	19	6	0	23	6	0	21	4	0	--	--	--	50	48	2	23
IV	3	1	1	4	2	0	2	5	0	10	1	1	7	2	1	14	6	0	60	60	0	8
V	3	1	0	3	1	0	3	0	0	3	0	0	2	0	0	4	1	0	60	60	0	3
VI	13	6	0	17	5	0	12	6	0	8	3	1	5	2	0	--	--	--	50	49	1	11
VII	6	2	0	8	2	0	8	2	0	10	2	0	17	3	0	--	--	--	50	50	0	10
VIII	3	1	0	3	0	0	3	1	0	3	1	0	3	1	0	7	2	0	60	60	0	4

O, observer.

T, average number of seconds required for the hallucination to arise.

MV, mean variation.

F, number of experiments in which no hallucination was produced within one minute.

M, total number of experiments.

N, total number of hallucinations out of M.

E, total number of no hallucinations within one minute.

A, average number of seconds for all T.

The object of using different signals was to find if the hallucination would be influenced by the character of the signal, but the conditions could not be satisfactorily controlled for that purpose, and the difference in time depending on this is not great enough to warrant any generalization from the figures on that point. It is, however, quite probable that this division and use of different signals tended to increase the force of the original suggestion by intensifying the expectant attention.

The leading mental process in these hallucinations[1] was an inference. The fact that the experimenter performed apparently the same manipulations that in the preparatory trials had produced a distinct sensation, formed the definite suggestion that, since the conditions were *in toto* repeated, the resultant sensation would recur in the same time and manner as before. By force of a firm expectant attention, caused by this inference, the image of the sensation realized itself in a sensation projected into the peripheral organs. And in the positive instances, the observer felt it just as he expected to feel it, although there was no physical stimulus.

Remarks like these were frequent: "faint," "distinct," "quite warm," "very distinct," "suddenly hot," showing that the hallucination existed in different degrees of intensity. It was difficult to convince those, to whom the experiment was afterwards explained, that the actual stimulus had really been withheld. The eighth observer had heard a lecturer refer to the test. When questioned after the test in regard to his experience, he said he felt the heat distinctly every time, although at first he thought there was some trick about it. From these experiments I conclude that a mental image of a definite liminal sensation of heat can be realized in a peripheral sensation in the absence of any physical stimulus if there is no incongruity in the phenomena which serve as suggestions.

Illusions of photometric changes in gray.

Apparatus.

This apparatus was constructed on the principle that the intensity of the shade of a white surface varies with the angle at which the rays of light strike it. The aim in its construction was to present two symmetrical white surfaces so adjusted that they could be turned independently at a graduated rate and distance, changing the angle of incidence of the rays of light that fell on both surfaces from one artificial light.

A front opening in a wooden frame, fig. 6, was covered by black card-board, which had two symmetrical openings, 2^{cm} apart. The

[1] I have observed the distinction between illusion and hallucination first made by Arnold (1806) and now accepted in a general way, namely, that sensory *illusion* is a false interpretation of an object, while sensory *hallucination* is a subjective sense image objectively realized by external projection; that is, in illusion there is a physical cause, while hallucination is purely subjective. It is, however, probable that there is a "point de repère" in many of the phenomena I have classified as hallucinations.

openings were 6^{cm} wide and 8^{cm} high. Back of them were two leaves swinging on hinges at the middle of the partition between the openings. The fronts of these leaves were covered with fine white Bristol board. To the outer ends of these leaves, cords were attached, which wound around an axle in the rear of the apparatus in such a manner that the motion of the leaves could be controlled

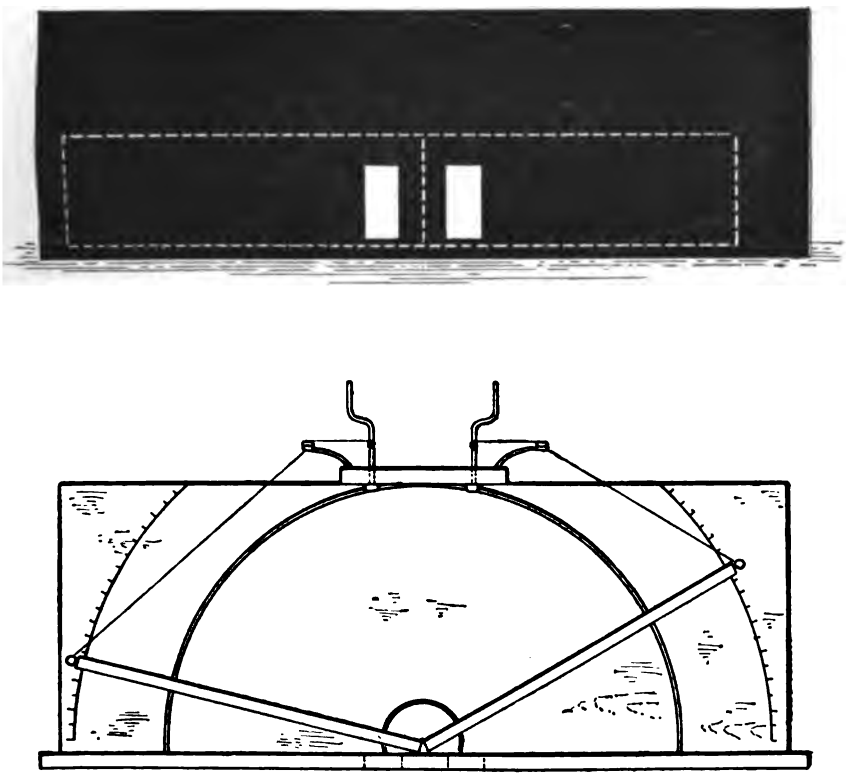

FIG. 6.

by a crank on the axle. One revolution of the axles constituted the standard for measurement of change in the angles, and was marked off in corresponding degrees on the arcs described by the outer ends of the swinging leaves. Thus the term degree is an arbitrary unit and not $\frac{1}{360}$ of a circle. For convenience, I will speak arbitrarily of the amount of change in the intensity of shade produced by the swinging of a leaf one degree in one second as a change of one

degree-second. The figures in all the tables of this section record the results in terms of such degree-seconds.

The apparatus was placed in the dark-room. A standard candle, one meter in front of it, lighted up both surfaces equally. The observer's chair was placed at a distance of 4^m directly in front of the apparatus. The range of vision was limited by two slits in card-boards, the one placed near the observer's eye, and the other 1^m away. By this means, distraction of the eyes was avoided and the tops and bottoms of the surfaces to be observed, where shadows were necessarily cast, were cut out from view. The surfaces were spoken of as right, R, and left, L, from the observer's point of view.

At the beginning of an experiment the white surfaces lay in the same plane and the incident rays of light struck them practically at right angles; i. e. the inner edges of the surface were 2^{cm} apart and the candle was placed directly in front of that partition. The angle of incidence decreased as a leaf was turned back on its hinge. The surfaces at no time appeared to be white but rather a light gray. The surfaces increased in intensity of gray inversely with the angle of incidence of the light rays. There was only one light in the room, and by the above arrangement all shadows were excluded from the surfaces in view. The crank was turned one revolution, i. e. 1 degree per second.

The apparatus was not constructed for very accurate photometric measurements, but for comparative measurements it serves very well.

Method of experimenting and results.

In the first series of experiments the rate of change was determined by the beating of a metronome. After a warning, R was turned 1 degree per second until the observer found it darker than L. Then L was turned until he thought L was equal to R. When this had been repeated a few times, I interspersed chances for illusions. That was done by simply allowing time after the warning without any movement of the leaves. The warning consisted in saying, "Ready," and the record was kept in seconds marked by the metronome. Nothing was said to the observer about the time of the discrimination. He was simply asked to state when he saw one surface lighter or darker or both alike. He could not see any manipulations of the apparatus. His attention was concentrated on watching the change of the shades. The fact of darkness and quiet

in the room heightened his expectant attention and threw him into a suggestible state of mind.

Several persons were experimented upon by this method with the result that the required change appeared to them universally from 6 to 13 sec. after the signal had been given, whether there was any real physical change or not.

Though the metronome which was used in the first experiments increased the force of the suggestion of change, it afforded an element of distraction, which was avoided in the later experiments by not using it. The reason for this was that the observer could not escape paying attention to the time, and this distraction might be so strong that after the first trial he would be in danger of interpreting a certain number of seconds as so much change of shade.

But even when the metronome was not used, the suggestion was so effective that the illusion could be produced alternately with the normal sensation.

In the following records S will denote the presence of a physical stimulus and I its absence. The column headed S in the table shows the number of degree-seconds of the change, i. e. the number of equal steps described on the graduated arc in an equal number of seconds. The column headed I indicates the illusions by giving the average time that it took them to arise. This time may be compared with the time given in column S.

TABLE VII.

Illusions of photometric changes in gray: Increasing and decreasing deepness of shade.

O	$S(o)$	MV	$S(25)$	MV	$I(o)$	MV	$I(25)$	MV
I	11.7	1.0	12.6	4.7	18.1	4.6	23.0	3.5
II	13.8	5.0	17.6	4.9	19.7	6.1	19.4	7.9
III	13.1	4.6	12.0	2.0	18.3	6.2	17.0	2.9

O, observer.
$S(o)$, number of degree-seconds the surface was darkened.
MV, mean variation.
$S(25)$, number of degree-seconds it was made lighter.
$I(o)$ and $I(25)$, corresponding illusions.

Can the illusion be produced in decreasing as well as increasing intensity of shade? The question was answered in the affirmative by the following test of which the final averages of the results are

given for three observers in table VII. L was left constant at 10 and R was moved toward 10 alternately from the 0 and 25 points, the number of degree-seconds indicated in the table and the observer was to tell when he judged the two surfaces to be alike. The *I* trials were alternated with the *S* trials.

In order to keep up the suggestion with some observers, I found it expedient to start moving the surface at the end of 20 seconds, if the suggestion had not then been effective. Hence the records higher than 20 contain only partial illusions, and, in rare cases, failures. Thus, in table VII, 23 shows that there was 3 degree-seconds of physical stimulus present.

TABLE VIII.

Illusions of photometric changes in gray: Dependence upon the number of repetitions.

O	*S* N=9	*MV*	*I* N=1	*S'* N=5	*MV*	*I'* N=5	*MV*
I	11	3	16	15	5	22	9
	13	3	30	12	3	23	7
II	4	1	7	6	1	8	2
	6	1	9	9	2	12	3
III	9	3	7	8	2	13	5
	11	1	10	13	2	15	2
IV	12	1	19	16	2	25	2
	15	1	24	12	1	27	2
Ave.	10.1	1.7	15.2	11.4	2.2	18.1	4.0

N, number of trials on each point. | Other notation same as before.

In accordance with the principle on which the apparatus was constructed, the deepness of the shade changed proportionally faster in the dark field. Thus, starting the leaves successively from 0, 10 and 20 and moving them at the same rate the change in shade would appear sooner after 20 than after 10 and sooner after 10 than after 0. Experiments on this point show that the time for the illusion to arise is proportional to the rate of actual change of shade, although the observer does not know that there is any difference in the rate of change.

The following method was used in determining the dependence of the illusion upon the frequency of the repetition of the suggestion in form of the real stimulus.

Four sets of ten trials each constituted an experiment. In the alternate sets the stimulus was given nine times successively and followed by an illusion, while in the other sets the stimulus was presented for the alternate trials and withdrawn from the rest.

L was kept constant at 0 and R was started at 0 and moved the number of degree-seconds indicated in table VIII, which gives the averages.

This shows that:

1. The required time and amount of change in intensity for discrimination is greater when the stimulus is given in alternate trials than when it is given 9 times in succession. Ratio, 11.4 degree-seconds to 10.1 degree-seconds.

2. The time for the illusion to arise is greater when the illusion is produced in alternate trials than when produced only once after 9 repetitions of the stimulus. Ratio, 18.1 seconds to 15.2 seconds.

I would not ascribe much value to these averages were it not that they express the relation which I find obtains, on the whole, in experiments on each of the other senses.

Time, it is evident, is the leading element in this kind of suggestion. By the following method I have secured a relative measure of its influence. The illusory element in the method of the next two experiments was the fact that, after giving a regular warning, I did not begin the movement of the leaf until after the number of seconds indicated at the head of each column.

The first observer had 100 trials and the second 40, but only the averages of the respective divisions are recorded in the table. The trials were so distributed with alternate *S* and *I* and a proper succession in regard to time as to eliminate systematic and progressive errors.

Since possibility of direct suggestion was precluded by the present method, the efficient cause in producing these illusions was simply the fact that the observer falsely inferred that the same change which took place after the first signals would recur after each succeeding signal. The warning or signal was given in such a mechanical way as to have no direct influence upon the discrimination.

The main results of these experiments on illusions of photometric changes in intensity of shade may be summarized as follows:

1. Visual illusions of deepness in shade can be experimentally produced by securing a firm expectant attention to a known sense percept;

2. Though time is one of the main elements in a suggestion, the illusion will occur in the absence of any *external* suggestion or special emphasis of passing time (i. e. the observer does not know that time is recorded);

3. A suggestion of change from dark to light shade is just as effective as the converse;

4. The time for the illusion to arise varies directly with the time for ordinary discrimination;

5. The discrimination-time and the illusion-time are both longer when the illusion is given in the alternate trials than when it is only given once after a series of ordinary discriminations.

TABLE IX.

Illusions of photometric changes in gray: Dependence upon the duration of the stimulus.

			5 *Sec.*				10 *Sec.*				15 *Sec.*				20 *Sec.*			
O	*S*	*MV*	*A*	*MV*	*D*	*I*	*A*	*MV*	*D*	*I*	*A*	*MV*	*D*	*I*	*A*	*MV*	*D*	*I*
I	16.3	2.7	17.2	2.4	0.9	4.1	19.0	2.6	2.7	7.3	22.3	2.5	6.0	9.0	24.9	1.6	8.6	11.4
II	10.5	1.6	12.0	1.5	1.5	3.5	15.0	3.5	4.5	5.5	17.3	5.2	6.7	8.3	15.5	5.2	5.0	15.0

S, regular stimulus present.
MV, mean variation.
A, result when the stimulus is delayed with the number of seconds indicated.
D, difference between *S* and *A*.
I, amount of the illusion expressed relatively in degree-seconds of change.
O, observer.

6. The amount of the illusion varies directly with the amount of variation in the time of producing the physical stimulus.

Illusions of light.

Apparatus.

The main part of this apparatus was constructed for the purpose of measuring the influence of the rate of change in determining the least perceptible difference in intensity of light, but has not before been described. It consisted of (1) a kymograph with attachment for rotating a wheel at a slow, measured rate; (2) a light with lenses, diaphragms, shutter, and reflectors; (3) a stop-watch, namely, the Runne chronometer with electric start and stop; (4) sounder and telegraph key in the dark-room.

An attachment was put on the Zimmerman kymograph for rotating a large card-board disk at adjustable, slow rates. On a radius of this disk, and in metallic contact with the frame of the kymograph, an arm projected with a platinum point, which made contact with another point insulated from the frame, at any position in the revolution. The edge of the disk passed between the two glasses of a double photographic lens. In this passage a diaphragm was inserted. It was 20^{mm} long on the inner edge, and the sides extended 25^{mm} on the radii of the disk. The rotating disk had an opening near the edge, measuring 80^{mm} on the inner side, with dimensions of the radii symmetrical with those of the opening in the diaphragm. The sides of the two openings coincided. The platinum point could be so adjusted that it made contact when the upper edge of the disk-opening was at any desired position in the diaphragm. There was another platinum point which could be so adjusted as to close contact when the lower edge passed any desired point in the opening of the diaphragm.

The light was cut out from the lens by a shutter which was supported by magnets, charged from a dip cell. Closing the platinum contact shunted this current through the apparatus, released the magnets, and dropped the shutter. The falling of the shutter opened the course for direct rays of light through the lens and at the same time closed a mercury contact, and thereby started the stop-watch.

An Argand burner was placed 50^{cm} in front of the lens ; 5^{cm} from this, between the burner and the lens, there was a white tissue paper, which diffused the rays of light. Two reflecting plates, having facing sides coated with magnesium oxide, were placed on the other side of the lens, parallel to each other, at an angle of 45 degrees to the rays of light. The light thus passed through the tissue paper, the lens, and the diaphragm, and was reflected by the coated surfaces. It was then seen in the dark-room through a circular opening 10^{mm} in diameter in a black diaphragm. There it appeared as a disk in the color of the gas-flame. Though it was very clear it did not have any excessive brightness.

Telegraph sounders were used for signals. The telegraph key in the dark-room was so connected with magnets on the stop-watch that the reaction stopped the watch. Hence the time between the dropping of the shutter and the movement of reaction could be read off on the stop-watch in fifths of a second.

The kymograph revolved the disk so that the opening in the diaphragm, between the lenses, was diminished or increased according as the lower or upper edge of the disk opening passed through it. The photometric principle involved was that the intensity of the stimulus changes with the variation in the size of the opening.

Methods and results.

While assisting another experimenter in tests on the threshold of change as dependent on the rate, it became apparent to me that suggestion might play an important part in the experiment. To determine this I repeated the experiment with the apparatus just described and by the same methods as were being used in the investigation referred to. I tried to keep all conditions similar, but put myself on guard to discover if any illusion was involved in the process.

The original problem was this: with the apparatus just as described, to find the dependence of the time and accuracy of discrimination upon the rate of change in the physical stimulus. The observer was to react as soon as he perceived any change in intensity of brightness of the disk. He knew that it was to grow darker at a definite rate.[1]

The running kymograph closed the lower platinum contact and thereby dropped the shutter the instant the lower edge of the opening coincided with the lower edge in the opening in the diaphragm. This act exposed the disk to view at its greatest intensity of brightness. But the continuous motion of the kymograph kept the cardboard disk revolving at a uniform rate. This regularly diminished the opening in the diaphragm, which caused a corresponding lessening of the intensity in the red disk, making it grow darker.

The observer was seated in the dark room, and received warning by a sounder 5 seconds before the shutter dropped. His instructions were: "React as soon as you see that the disk has become darker than it was at its first appearance."

It was not stated that the change would always be the same, but the observer generally inferred this when he saw the apparatus.

In sets of 20 observations each, I interspersed chances for illusion, at irregular intervals, by simply stopping the kymograph just as the

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[1] Scripture, *On the method of regular variation*, Am. Jour. Psych., 1891 IV 577.

Scripture, *Ueber die Aenderungsempfindlichkeit*, Zt. Psych. Phys. Sinn, 1894 VI 472.

disk was exposed. It was thus kept constant at full intensity of brightness. These furtive interspersions were made from 2 to 6 times in the 20 trials, and the observers almost invariably reacted to these corresponding *expected* changes, perfectly confident that they had perceived real changes taking place.

After the fact of this illusion was known, two persons continued the experiments on the threshold, for which this apparatus was originally intended. To test the accuracy of the discrimination the experimenter interspersed chances for illusions as in the above method. These illusions were never detected by the observer. For the purpose of the original problem, the experimenter had to agree not to indulge in any such tests because, after having been told of it, the observer became suspicious that it would be repeated, and was made over-cautious. This over-cautiousness led him to wait and verify his first judgment, and that made the discrimination-time worthless.

TABLE X.

Illusions of light.

O	*LS*	*LI*	*DS*	*DI*
I	3	3	5	3
II	8	8	5	9
III	5	6	5	4
IV	4	6	3	5

O, observers.
LS, disk grew lighter (actual physical change).
LI, disk grew lighter (no physical change).
DS, disk grew darker (actual physical change).
DI, disk grew darker (no physical change).

The record is kept in seconds. Each figure is the average of five observations.

To find whether the illusion could be obviated in an experiment of this kind, I instructed the observer to react as soon as he could tell definitely whether the disk was growing lighter or darker.

The two platinum contacts were so adjusted that, by closing the circuit with either one, the disk would be exposed at half its possible intensity. When the lower contact was used, the disk would continue to grow darker, and when the upper contact was used, it would continue to grow lighter. The results are shown in Table X. In this and the following tables of this section the unit of measurement is the amount of change in a second at the given rate. We may again call this a degree-second of change.

As these figures indicate, the illusion came out surprisingly strong even when the precaution was taken, to require the observer to discover the direction of the change. Though there was no suggestion as to whether the disk should grow darker or lighter, the firm expectation that a change would occur one way or the other proved a sufficient cause to call forth the illusion.

Although these were bright men and very cautious, they exemplify the very extreme degree of suggestibility to this kind of illusion.

For the present purpose it was unnecessary to make exact photometric measurements, because these relative determinations suffice to bring out the laws of the suggestions at work.

The apparatus was so constructed as to admit of a great variety of graded rates of change. In units of the arbitrary gradation, 0.22 per second was found the most favorable rate for effective suggestion. At this rate, which is employed hereafter, the entire range of change that the apparatus permitted was traversed in 16

TABLE XI.

Illusions of light: Rate of change.

	I		II		III	
Rate per sec.	*LS*	*LI*	*DS*	*DI*	*LS*	*DG*
0.22	7	12	7	9	8	5
0.33	8	13	6	10	7	4

Notation same as in the preceding table. Each number is the average of 5 observations.

seconds. If, e. g., twice that rate was used, there was danger of either hasty, automatic reaction or inhibition of the illusion. And, if the rate was made extremely slow, the result would generally be a confusion because the transition was so gradual that it was difficult to retain a distinct memory image of the original impression.

Table XI records the averages of an experiment in which the illusions with two different rates of change are compared. In the first set the rate of change is 0.22 per second, and in the second set 0.33, i. e. the latter is proportionally slower. Before beginning the experiment the observer was given a few preliminary trials—not more than ten, and thereupon he was instructed : In the first set, react when the disk grows lighter ; in the second, when it grows darker ; and, in the third, when you can see whether it has grown lighter or darker. The exceptionally small difference in time for the two rates is partially explained by the fact that the latter

set of observations was made soon after the former, which undoubtedly served as a suggestion, shortening the time in the other. This fact emphasizes the importance of arranging laboratory experiments in a proper order of succession.

To discover whether the illusion could be worked up in persons who were particularly on guard by expecting an attempt of illusion, I made the following test: I selected two observers who knew that I was experimenting on illusions. I had previously tried them with a slightly different method and failed, but now nothing was said in regard to what would take place. The instructions were, in I and V (Table XII), to react when the disk had become perceptibly darker; in II and IV, when it had become lighter; and in III, when the observer could tell whether it had grown lighter or darker.

TABLE XII.

Illusions of light: Development of the suggestion.

	I	*II*	*III*		*IV*		*V*	
O	*DS*	*LS*	*LS*	*DS*	*LS*	*LI*	*DS*	*DI*
I	12.2	5.4	2.3	2.6	4.8	6.2	6.6	11.0
II	13.2	5.5	9.0	2.2	6.5	8.6	6.0	7.4

The notation is the same as in preceding tables. The figures are averages of ten trials in each group.

This shows how the suggestion accumulates force by successive repetitions of the real stimulus. As was shown in other cases, the illusion would not have been likely to occur had the chances for illusion been introduced near the beginning. This is also shown by the long discrimination-time in set I.

Here we can trace the evolution of an illusion. In I the observers were on their guard, very suspicious, and did not react upon the first perception of change. In II they grew more exact, and by this time they began to convince themselves that no illusion was involved. III is an index to their reliability. In IV and V the alternate trials are illusions and here these two observers showed themselves just as susceptible to the suggestion as those who were not thus prejudiced.

Pursuing the method employed in the experiments recorded in Table IX, similar measurements were more extensively carried out with the present apparatus, giving the results of which the averages are quoted for eight observers in Table XIII.

The observers were required to react when they could see that the disk had grown brighter. The figures at the heads of the

columns show how many seconds the application of the stimulus was delayed after the warning. The omission of the stimulus was made by stopping the kymograph for so many seconds, allowing one second for it on which to start.

Progressive errors were avoided by the distribution of the trials according to this method, one trial in each group being made in succession back and forth, with *A* and *I* alternating. Since only two trials were made on each point, the variation in the mean results is quite large.

TABLE XIII.

Illusions of light: Dependence upon the time of application of the stimulus.

			5 *Sec.*				10 *Sec.*				15 *Sec.*				20 *Sec.*			
O	*S*	*MV*	*A*	*MV*	*D*	*I*	*A*	*MV*	*D*	*I*	*A*	*MV*	*D*	*I*	*A*	*MV*	*D*	*I*
I	14.3	2.2	13.5	1.5	—0.8	5.8	21.0	1.0	6.7	3.3	28.5	6.5	14.5	0.8	29.0	6.0	14.7	5.3
II	6.2	1.9	6.0	1.0	—0.2	5.2	14.5	2.5	8.3	1.7	23.5	3.5	17.3	—2.3	10.5	2.5	4.3	15.7
III	6.6	1.4	7.5	0.5	0.9	4.1	8.0	1.0	1.4	8.6	10.5	4.5	3.9	11.1	7.0	0.0	0.4	19.6
IV	5.0	2.0	6.5	1.5	1.5	3.5	4.0	0.0	—1.0	11.0	6.5	0.5	1.5	13.5	6.5	0.5	1.5	18.5
V	9.6	2.4	9.5	1.5	—0.1	5.1	15.5	0.5	5.9	4.1	19.0	11.0	9.4	5.6	25.5	0.5	15.9	4.1
VI	5.0	0.9	6.0	0.0	1.0	4.0	6.5	0.5	1.5	8.5	6.0	0.0	1.0	14.0	6.0	0.0	1.0	19.0
VII	6.0	1.4	6.0	1.0	0.0	5.0	6.0	0.5	0.5	9.5	8.5	1.5	2.5	12.5	7.5	0.5	1.5	18.5
VIII	7.0	0.9	7.5	0.5	0.5	4.5	9.5	0.5	2.5	7.5	7.0	0.0	0.0	15.0	14.0	6.0	7.0	13.0

S, regular stimulus present.
A, stimulus delayed the number of seconds indicated at the head of each section.
D, difference between *S* and *A*.
I, amount of the illusion.

The results of these eight experiments may be divided into two classes according to the general laws which they express: (1) those in which the omission of the stimulus does not increase the discrimination-time to any marked degree, namely III, IV, VI, VII, and VIII; and (2) those in which the discrimination-time was increased nearly proportionally to the time the stimulus was omitted, namely I, II, and V.

In the first class we observe that the time-suggestion was complete or nearly so. The observer looked at the disk and saw it grow brighter when there was no physical change in the intensity of its brightness, in nearly the same time as when there was an actual physical change.

In the second class, the illusion occurs sometimes as in the first, but when it does not occur the discrimination-time is shortened. This shortening may then be considered as a partial illusion, which is expressed in column *I*. There is only a difference of degree between the two classes. All observers were more or less deceived. The comparative extent of the illusion can be seen by a glance at column *I* in each section of the table.

There are especially two reasons why this colored disk was so efficient in producing the illusions. First, a memory image of the first intensity had to be compared with a later intensity. A memory image of color is, however, easily retained as long as was here required. But, secondly, this was made difficult by the *gradual* transition. A gradual change must be much greater than an abrupt one to be perceived.

Since the whole range for change in intensity of the light at the rate employed was traversed in 16 seconds and the light was presented when it was at half intensity, it took only 8 seconds to bring it to its full intensity, where it remained constant. Hence those who required more than 8 seconds for discrimination perceived the difference when the intensity had actually been constant for some time.

Besides the general fact of the existence of the illusion, the following four points have been established by this series of experiments.

1. Suggestion, as a neglected quantity, vitiates a vast amount of laboratory experiments on perception of liminal differences.

2. The influence of suggestion is not always avoided by requiring the observer to discriminate between two stimuli, either of which may appear, because he may unconsciously look for only one of them or by some trivial circumstance suggest to himself that a particular one of the two will appear ; and if sufficient time be allowed, the illusion may occur just as if one definite stimulus had been expected.

3. If an observer begins an experiment warned against illusion and determined not to be deluded, this negative suggestion may be overcome by repetition of the real stimulus, and the positive suggestion will delude him as if he had been unwarned.

4. Discrimination for liminal differences depends largely on the regularity with which the physical stimulus is applied after the warning. If the stimulus be unexpectedly delayed, the observer will in some instances perceive smaller objective differences than ordinary, but more frequently he falls into an illusion, partial or total.

Hallucinations of an object.

Most of my experiments have been designed to produce hallucinations or illusions of single qualities or differences in qualities of objects. Can hallucinations of complete objects be produced in the same way? That they can, may be positively inferred from the results already obtained, but I have made a simple test to prove it directly.

The object, of which a hallucination was to be produced, was a blue bead, spheroidal, the shortest diameter being 1.8^{mm} and the longest 3.5^{mm}. It was suspended by a fine black silk thread in front of the center of a black surface, which was surrounded by a white circular border whose inside diameter measured 50^{mm}. By a concealed device, the bead could be drawn away and replaced without the observer's notice.

The apparatus was placed on the wall facing the door in the dark room, so that when the door was open the observer could walk up an aisle in front of the door, constantly having the apparatus in line level with his eyes. The experimenter was seated by a table in the dark room, ostensibly to keep record, but really in order to manipulate the apparatus. The tests were made during daylight; to avoid the shadows of the dark room and secure a fairly constant light, an incandescent light was kept burning in the ceiling of that room. It was essential that the light should be fairly constant only during one experiment.

A tape line was stretched from the apparatus to a point 6.5^{m} directly in front of it. The method employed was to first show the observer the bead in its position, then require him to go to the further end of the tape line and walk slowly up towards the apparatus until he could first see the bead distinctly. When he saw the bead, he read off the distance from the apparatus on the tape line. I recorded the distance while he went back to repeat the trial 19 times in the same way.

In the first ten trials, the physical conditions were similar and he saw the bead at different distances with but a small variation. While he went back to start for the eleventh time, I pulled a cord which slid the bead behind the frame. The observer, not knowing this, walked up as usual, and when he came to, or a little beyond, the point where he expected to see it, he generally *did* see it, and read off the distance as before. While he looked at the tape line I slid the bead back in place.

As a rule, the eleventh, sixteenth, eighteenth and twentieth trials were made with the bead withdrawn.

I did not make the test statistical at all, though it is simple and well adapted for examination of great numbers of individuals if due precaution be taken.

About two-thirds of the persons I tried were hallucinated. They knew *when*, *where* and *how* to see the bead, and this was sufficient to project the mental image into a realistic vision. This qualitative result was all that I desired to obtain and I carried the experiments no further.

Hallucinations of sound.

There are three prominent difficulties that confront us in trying to produce hallucinations of sound experimentally in normal life. (1) It is almost impossible to separate the required sound from other external sounds and from the subjective, or entotic, sounds. (2) Regulating and measuring the physical stimulus does not regulate and measure the resultant sensation. (3) It is difficult to induce a firm expectation of the sound by circumstantial suggestion.

The immediately following tests were of a precursory nature, and were not carried out with any scientific thoroughness, but some of the general observations may be of interest.

A. Preliminary tests.

1. The graphic method of recording[1] was used, and the observer occupied the dark room. After a regular warning (one tap on an electric bell) faint clicks were given on a telegraph sounder placed on a table by which the observer was seated. There was no strong resemblance between these clicks and the sound of the bell. The warning was repeated every six seconds, and three clicks were given in rapid succession two seconds after the warning. The suggestion, induced by experience, was that these sounds would recur regularly in the same order. When, however, the experimenter caused an exception to this order, by occasionally not giving the clicks after the warning, the sound of the warning frequently directed expectant attention so strongly to the sequel, that the observer thought he heard the clicks also.

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[1] Bliss, *Investigations in reaction-time and attention*, Stud. Yale Psych. Lab., 1892–1893 I 1.

The time between the warning and the stimulus was made as long as two seconds to avoid the probability of automatic reaction.

The clicks from the sounder were too unfamiliar and strange, and hence difficult to reproduce. They had to be made so faint that the observer did not feel certain about his discriminations.

2. Instead of this stimulus I tried the efficiency of a click produced in a telephone by closing the circuit. This had the same difficulty, and the additional disadvantage of being definitely located in the diaphragm of the telephone.

3. The effect of a constant sound from a tuning fork was then tried. A 250 v. d. fork was kept vibrating before a telephone in a distant room, and by means of resistance, the sound received at the telephone in the dark room could be so adjusted that, allowing for all fluctuations, it would still be decidedly above the threshold of perception.

The observer was required to tap a key at a convenient rate, in circuit with the recording drum, as long as he heard the sound. It is a normal experience that sounds or tones of this character are readily reproduced as hallucinations, but for experimental purposes I could not get a suggestion definite enough.

Instead of inducing a conviction that the sound was continuous, the very fact that the observer was asked to react as long as he heard the sound intimated that there would be an interruption. Notwithstanding this vagueness, several observers were so strongly hallucinated, that when the stimulus was withdrawn, they still kept on reacting until stopped. The stimulus had been applied long enough to produce a definite sensory image of the sound; when the stimulus was withdrawn an after-image of it persisted for a moment, then the hallucinatory sensation persisted the better, because there was no contrasting sound to disturb or rectify it.

4. In order that an assurance of the continuity of the sound should be resolutely induced by auto-suggestion, I next tried the tick of a clock for stimulus. The sound was transmitted from the clock to the dark room by the Blake transmitter with telephone connections in which an open-circuit key was inserted. The intensity of the sound was regulated by resistance so as to be above the threshold of distinct perception when the observer's attention was at its lowest point while he was listening.

The advantage of this stimulus was that the tick of a clock is familiar and a memory image of it is easily retained. It was reg-

ular, definite and continuous. The observer knew I was recording his reaction, but he did not know how or why.

Without the use of any warning, he was asked to give four sets of reactions, namely: (1) every second tick, (the clock ticked 80 times per second); (2) every fourth tick; (3) every fifth tick; (4) every tenth tick. By these reactions the beats would be grouped with accent as in musical time. The observer would fairly throw himself upon the rhythmic movement, and, as he surrendered himself to it, a constant inhibiting effort had to be exerted in order to keep the reaction from anticipating the tick and gradually accelerating the rate.

It has been observed by MÜNSTERBERG[1] that a brief interruption in a stimulus of this kind will be bridged over. The same principle was involved in the experiments of URBANTSCHITSCH[2], N. LANGE[3], ECKENER[4], PACE[5], MARBE[6], and others who studied the fluctuation of attention to liminal sensations.

Frequently, in my experiments, from 1 to 10 ticks could be omitted and the observer would bridge it over. But, what is more remarkable, the hallucinatory sensations would sometimes rule out the sensation directly caused by the stimulus, so that the observer would e. g. react for 16 ticks while in reality only 12 were produced in that time.

Such hallucinations may be due to the tendency in an observer to accelerate any rythmic movement as it progresses.

5. The next apparatus was constructed so as to produce a given sound at regular intervals. Two contacts on the kymograph were so connected with magnets on a stop-watch that when the first contact was made one magnetic armature was drawn, closing the circuit which included an isolated 250 v. d. tuning fork and the primary

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[1] MÜNSTERBERG, *Schwankungen der Aufmerksamkeit*, Beitr. zur exp. Psych., Vol. I, No. 2 p. 69.

[2] URBANTSCHITSCH, *Zur Lehre von der Schallempfindung*, Archiv f. d. ges. Physiol. (Pflüger), 1881 XXIV 574; *Ueber subjectiven Schwankungen der Intensität akustischer Empfindungen*, Archiv f. d. ges Physiol. (Pflüger), 1882 XXVII 436.

[3] LANGE, *Beiträge zur Theorie der sinnlichen Aufmerksamkeit und der activen Apperception*, Phil. Stud., 1888 IV 390.

[4] ECKENER, *Untersuchungen über die Schwankungen der Auffassung minimaler Sinnesreize*, Phil. Stud. 1893 VIII 343.

[5] PACE, *Zur Frage der Schwankungen der Aufmerksamkeit nach Versuchen mit der Massonschen Scheibe*, Phil. Stud., 1893 VIII 388.

[6] MARBE, *Die Schwankungen der Gesichtsempfindungen*, Phil. Stud., 1893 VIII 615.

coil of an inductorium ; and when the second contact was made the other armature was drawn, breaking the circuit. The secondary current from the inductorium completed a circuit through telephones in the dark room.

The kymograph was run at the rate of one revolution in ten seconds, and the contact points were opposite each other ; hence, the sound was sent through for five seconds and then interrupted for five seconds. In this way a uniform alternation could be continued indefinitely by keeping the kymograph wound at a constant tension.

The closing of the circuit, which produced the stimulus, started the stop-watch, and the observer's reaction stopped it. The time and regularity of the reaction were determined by the indications of the watch.

Two telephones were similarly connected in the same circuit and held one to each ear. When they were similarly adjusted, the sound was localized at the root of the tongue in the median plane of the head ; and, according as one was pressed relatively closer to the ear, the sound was localized to that side. But the contact of the telephones with the ear caused a disturbance, and it was difficult to adjust the two alike. Therefore the two telephones were permanently fixed facing each other, 150^{cm} apart, in such a position that when the observer was seated his head would be midway between the two. The advantage of this was that the sound seemed to fill the room and could not be definitely localized.

The aim of the experiment was to produce hallucinations of this sound by repeating it until the observer had taken up the rhythm and acquired a clear mental image of its character.

The observer was required to react every time the sound recurred. After he had continued to do this regularly a short while, the sound was occasionally cut out by means of a switch. If a hallucination had been aroused, the observer would continue to react, as was frequently the case, for over a minute, when there was no stimulus present.

But here again the very suggestion raised a doubt in the mind of some observers ; and, by actual experiment, I found that the discrimination for sound is finer when the sound is interrupted at intervals, as here, than when it is continuous. Therefore this method was also abandoned after some trials, and the real experiments I carefully carried out were made by the apparatus and method now to be described.

B. Main experiments on auditory hallucinations.

The apparatus for this series of experiments was so constructed as to produce a sound whose quality was constant, and whose intensity could be varied at a desired rate.

A 250 v. d. tuning fork, in an insulated box, was kept vibrating in the primary circuit of an inductorium. The induced current was completed through a telephone circuit in the dark room and could be broken by a switch. The telephone was fastened near the ceiling in such a position that it was 75^{cm} back of, and 150^{cm} above, the ears of the seated observer. This secured the same effect as the two telephones in the preceding arrangement, and the sound seemed to be distributed throughout the room. The quality of the sound was not a tone, but rather a distinct buzz, the result of the interrupted current. Telegraph sounders were used for signals, one in the dark room and one in the experimenter's room.

The method pursued can be best understood by examining the collective records in Table XIV with the following explanation in mind.

The observer occupied the dark and quiet room (a room with double padded walls supported on rubber disks so as to exclude sight and sound disturbances).[1] After I had given him a trial signal and produced the stimulus for his recognition, the instructions were essentially: "The signal means, *Listen;* every time you receive it, listen attentively until you hear the sound, and when you first perceive the stimulus, react. That reaction will indicate the threshold for your perception of sound."

The intensity of the sound was regulated by means of the inductorium. When the secondary overlapped the primary coil the indicator pointed to zero, and the sound was at its greatest intensity. As the secondary coil was moved back from the primary, the intensity of the sound decreased in a ratio favorable to the relative measure required. The figures in the record give the number of cm. by which the coils were separated. This is made the arbitrary measure of the physical stimulus.

At the moment the signal was given, the secondary coil was started so far down the scale that the sound produced would be below the threshold of perception, and then moved toward zero at

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[1] Bliss, *Investigations in reaction-time and attention*, Stud. Yale Psych. Lab., 1892–1893 I 2.

the regular rate of 0.5^{cm} per second. Thus if the starting point was 25^{cm}, and the point when the reaction occurred 20^{cm}, it required 10 seconds to reach the threshold.

When the stimulus had been given about ten successive times, approximately once in 20 seconds, I continued to give the signal regularly, but occasionally cut out the sound with the switch before

TABLE XIV.

Hallucinations of sound.

O	*P*	*S*	*MV*	*NS*	*H*	*MV*	*NH*	*F*
I	25	20.8	0.6	3	14.0	....	1	....
II	27	21.6	0.8	15	....	....	....	2
III	25	22.0	0.9	16	20.8	0.3	2	1
IV	25	21.5	1.0	14	....	....	....	2
V	25	21.9	0.8	12	20.9	1.9	6	....
VI	25	22.6	0.8	17	....	....	....	3
VII	27	18.2	1.1	29	20.0	3.0	2	3
VIII	30	25.6	1.8	11	19.0	....	1	....
IX	30	24.6	1.5	16	24.3	2.9	3	....
X	27	18.9	1.3	18	....	....	....	2
XI	27	21.3	2.1	17	20.0	2.0	3	....
XII	25	21.3	1.8	17	22.5	0.3	3	....
XIII	26	23.2	0.7	17	24.2	0.8	3	....
XIV	32	27.5	0.9	48	28.5	1.0	8	3
XV	30	19.8	0.4	11	....	....	....	1
XVI	28	23.2	0.8	17	....	....	....	3
XVII	30	20.7	0.6	17	....	....	....	3
XVIII	30	25.1	1.2	18	24.0	....	1	1
XIX	30	23.0	1.1	16	22.0	....	1	2

O, observers.

P, point on the scale of the inductorium at which the indicator was started.

S, point at which the stimulus was perceived.

MV, mean variation.

NS, number of trials in *S*.

H, point at which the hallucination was realized.

NH, number of hallucinations in *H*.

F, number of failures to realize the hallucination in the limited time.

the signal was given. I moved the indicator up the scale at the same rate as before, in order to get an expression for the time-relation of the hallucination, which could be interpreted in terms of the rest of the record.

Table XIV contains the record of the experiments on 19 persons, quoted in the order the tests were made ; negative as well as positive results. It is not merely a statistical fragment, but is intended to give the reader an opportunity to trace the actual procedure and experience in a test of this kind.

To explain the table, 25 at the head of column *P* in the record of observer I means that the secondary coil was started 25^{cm} down the scale from the primary coil every time the signal was given. 20.8 in column *S* indicates (1) the relative intensity of the sound when first heard, and (2) the time required for the stimulus to rise to this intensity after the signal. The higher the number, the weaker the sound it represents, and vice versa.

We may designate the movement of the secondary coil, 0.5^{cm} per second, as an increase of one degree-second in the intensity of the stimulus. Thus, on the average, observer I perceived the sound after an increase of 8.4 degree-seconds from the starting point ; observer II, 10.8 degree-seconds, etc. No sound was produced physically, still observer I heard it 22 sec. after the signal. 2 in column F means that in that experiment the physical stimulus was omitted twice, but no hallucination was produced in the time allowed, namely, 30 sec. When no hallucination had arisen at that time, the stimulus was applied with such an intensity that it could at once be perceived in order to keep up the suggestion, and such trials were properly recorded as failures.

After each experiment it was ascertained whether the observer had been warned by any person or circumstances in any way to affect his judgment. It was found that nearly all those who gave negative results had been cautioned in some way to be suspicious, but this distinction between those who were warned and those who were not warned cannot be sharply drawn.

The psychological laboratory is something comparatively new to everybody. Persons come in there to see new things and are prepared for surprises, if that be possible. Since I did not tell any person that such and such a sound will recur so many times at such and such a rate, but left him to work up the auto-suggestion for himself, it may be reasonably supposed that every observer was in some degree sceptical as to the outcome of an experiment. The extent of this sceptical reserve very largely determined the degree of success in the experiment.

To shut a person up in a dark room is to intimate that he should pay attention to sounds. Then the associations with the signal tune

up the sensorium to such a tension that the experience of previous sensations is vividly imaged and the observer, as it were, feels and pictures the stimulus approaching the threshold, just as a batter feels and sees the ball approaching him after it has left the hand of the pitcher. For, by the method employed, a strong association was set up between the expected sound and the spatial and temporal environment.

From this point of view, we may trace in the positive and negative aspects of the results two co-ordinate elements ; first the degree of expectation, and second the power of vivid imagination. I am confident that if it were required to select a number of persons who could be systematically hallucinated according to the present method, it could be done by means of a careful preliminary examination of them in these two respects.

The measure here is only relative and arbitrarily chosen, depending upon the law of increase in intensity of an induced current, but it serves the purpose quite as well as if the physical measurements were absolute. The threshold is never the same for different individuals, nor is it constant for the same individual at different times. The aim in view here was to produce a hallucination of a sound such that the observer could say positively, "*I hear it.*" The intensity of such a sound was determined by the delicacy of the observer's auditory apparatus and his accuracy of discrimination.

Of the 60 attempts to hallucinate, 34 were successful and 26 unsuccessful, within the time-limit allowed. This is a large proportion of affirmative answers, when we consider that those who were forewarned are included.

After the hallucination once is started it is easy to continue it, and each successive hallucination contributes toward the building up of a firm expectation, just as the repetition of the real stimulus does.

Thus, if there are three successful hallucinations and no failure in the first 20 trials, it is very probable that with such an accumulation of positive associations the experiment might with most observers be successfully continued through a series of trials without any stimulus. I made several trials on that point in addition to those recorded in Table XIV. As a rule, the hallucination would be kept up as long as the observer could continue unabated attention to it, and it would be rectified as attention slackened from fatigue. But for some observers the hallucination would continue indefinitely ; i. e. so far as my experiments went, for when they had been hallucinated in 20 or 25 successive trials, the experiment had to be discontinued on account of fatigue.

These experiments forcibly show the difficulty of exact measurement of mental activities and the danger of getting false results even from the most faithful observer. Had all these results been positive, it would have pointed to a despairing state of affairs with regard to our ordinary reliance upon the senses. The experimental psychologist will recognize in the method here used (excepting the omission of the stimulus) a familiar type of experiment on discrimination, which is generally taken in good faith. These tests prove it absolutely unreliable; one little circumstance may determine what the observer shall hear or not hear without regard to the physical stimulus.

To determine the effect of repetition, I made the above experiment with one reliable observer, under similar circumstances, at ten different times. Each experiment consisted of twenty trials in which the stimulus was regularly applied in the first ten and omitted three or four times in the last ten trials.

These ten records show: (1) He discriminates for lower intensities in the latter part of each experiment. (2) Similarly he discriminates for lower intensities in the last than in the first experiments. (3) The hallucination is successfully produced in all but one of the thirty-seven trials. (4) The average reaction time to the hallucination is slightly longer than the reaction time to the real stimulus.

That the reaction time is shorter as the experiment progresses, may be partially accounted for by the fact that time is more and more over-estimated during a continuous strain of attention.

Using these ten experiments as a suggestion, I made a final experiment of twenty trials in which the stimulus was never applied. In each trial the observer signalled that he heard the sound, and after the experiment was over he was very confident that he had perceived the sound distinctly.

Beyond the details discussed, these experiments on hallucinations of sound have demonstrated two points:

1. Hallucinations of sound distinctly above the threshold can be produced experimentally in normal life by leading the observer to concentrate expectant attention upon the desired result.

2. Experiments to determine the threshold for perception of sound cannot be continued through a series of repeated trials, without being vitiated by the suggestion due to the accumulating associations.

Hallucinations of touch, taste, smell and electric stimulation.

These tests are grouped together because they are of a rudimentary nature and depend to a great extent upon the same principle. There were three main points in the aim of the method: (1) that the observer should clearly recognize the sensation to be produced; (2) that by repetition it should become familiar and definitely associated with its cause; (3) that the observer should be led to watch expectantly for it in the accustomed time and manner.

Touch.

I will mention in a cursory manner the methods by which I succeeded to produce hallucinations of touch. Pithballs were used for stimuli. The size and weight had to be adapted to the cutaneous sensibility of each individual, as well as to the place chosen for stimulation. I did not attempt any definite measurement of the size, weight or momentum of the impinging disk, but was satisfied when the force of the stimulus was such that the observer could say that he distinctly felt the sensations produced.

According to the first method, the instructions were, "Say 'There' every time I touch you with the pithball at this point" (e. g. the knuckle of the middle finger where there are no hairs).

The regular stimulus was then given six or eight times to build up the suggestion. After that it was occasionally omitted, i. e. according to the accustomed rate at which it was expected. The observer was seated behind a screen, and the experiment was tried with and without warning. When no warning was used, the first stimuli were given at a constant rate, about once in five seconds.

Some of the observers presented the ludicrous spectacle of sitting behind the screen, actually feeling the sensation and confidently repeating, "There, there, there," etc., for an indefinite time after the apparatus used for stimulus had been laid aside.

When the warning, "Now", was used the stimulus was applied at irregular intervals. In this case time was no object in the reaction, hence the observer was not rushed to give a hasty judgment. Yet in about three-fourths of the cases, the mere warning was sufficient to produce the sensation of touch.

In a variation of this method, the observer was instructed to tell *when* he was touched after each warning. The place of stimulation was limited to the back of the right hand and wrist. I would

actually touch him about ten times and then continue to give the warning without touching him. More than half of the observers were able to point out the place where they felt the hallucinatory sensations. That these sensations were above the threshold is shown by the fact that when the stimulus was present the observers could localize it correctly, and on inquiry it was found that the hallucinatory sensations sometimes appeared to be more distinct than the regular sensations.

The suggestion was increased by attaching the pithball to the pendulum of a metronome, by means of a cocoon fibre, so that every time the metronome ticked, when the pendulum was on one side, the ball would drop on the hand of the observer. When it had touched about ten times I caught the ball so that it did not touch the observer, though the metronome continued to tick as usual.

The association of the touch with the beat of the metronome strengthened the suggestion and made the results more definite and almost universally positive. The remarks "distinct", "very distinct", etc., about the hallucinatory sensation were frequent.

The most successful suggestion produced by associating the touch with a sound was, perhaps, the following:

The fibre on which the pithballs were suspended was attached to a special lever on the stop-watch, in such a manner that when the experimenter moved the lever, it simultaneously started the watch, and dropped the pithball. The observer's hand was comfortably adjusted in a fixed position with the palm up. He sat behind a screen and could stop the watch by his reaction on a key. The tick of the watch was so loud that he could hear it by paying attention.

In the first ten trials he had learned that the watch began to tick just as he felt the ball touch, and suggested to himself that this association was permanent. Hence, when after the tenth trial the ball was removed, he argued that every time he heard the tick of the watch again the touch would recur with it, and it actually did, so far as most of the observers' sensations were concerned.

Taste.

An electrical stimulus was used in the first test on hallucinations of taste. The intensity of the induced current was regulated by means of changing the distance between the two coils of an ordinary inductorium. One of the electrodes was furnished with a platinum sheet 8^{mm} square, which the observer applied to the tip of his

tongue, holding the other in his hand. The observer was asked to react when he could perceive the acid taste of the current. He stood by the apparatus and saw me start the secondary coil at such a point, that, when moving it at a regular rate toward the primary coil, the threshold for perception of the current would be reached in about ten seconds.

This was repeated ten times, but the eleventh and following trials I cut out the current with a secret switch. The observer had by this time associated the cause with the effect, and elaborated the image of the sensation so thoroughly that he generally tasted the current in the expected time and manner.

Gustatory hallucinations were also produced by associations with liquid stimulation. The test is rude but it illustrates a principle.

Of six bottles, two contained pure water, and the other four a series of solutions of pure cane sugar ; the first ½%, the second 1%, the third 2% and the fourth 4% sugar, according to weight. A block was casually placed in front of them, so that the observer could not see them although he was aware that they stood near by him, because he saw them when he received his instructions. It was required of him to tell how weak a solution of sugar he could positively detect. I took a glass dropper and deposited a few drops on his tongue, drawing first from the two water bottles, and then from the sugar solutions, in order of increasing strength. The sugar was detected in the ½% or 1% solution the first trial. Proposing to repeat the test, I proceeded as before, but drew from the first water bottle every time. The result was that when the pure water had been "tasted" from two to ten times the observer almost without exception detected sugar.

It was not a persistence of the taste from the first trial, because in each repeated test it was not perceived at first.

From the judgments passed, it appeared that, though the dropper was filled from the same water every time after the first trial, the liquid grew sweeter and sweeter to the observer, until he could confidently say that there was a decided taste of sugar.

Smell.

The test on olfactory hallucinations was conducted similarly to the test last described.

Twelve bottles containing solutions of oil of cloves were arranged in such a series that the first contained fresh distilled water, the

second $\frac{1}{1\,000\,000}$ part of oil of cloves, the twelfth $\frac{1}{1\,000}$ part of oil of cloves, and the intervening ones were arranged in geometric ratio according to strength.

Ordinarily the oil of cloves could be detected in some bottle between the third and the ninth from the water. I showed the observer these bottles in a rack and then asked him to stand in the fresh air by an open window in the adjoining room, while I brought and let him smell of the liquid from one bottle at a time. Since he was asked to tell in what bottle he could first perceive the smell of oil of cloves, he inferred that I would begin with the weakest solution and then take them in order of strength, though nothing was said to him about that. Some time before the real experiment, I let him smell of the solutions at random, so that he knew definitely what to expect, but in what he considered the test, I brought him the *water* bottle every time.

About three-fourths of the persons experimented upon perceived the smell of oil of cloves from the pure water bottle when it had been brought from three to ten times.

Electrical stimulation.

For this apparatus I used the inductorium with the 250 v. d. tuning fork vibrating in the primary circuit.

The electrodes of the secondary current terminated in the bottoms of two tumblers, filled with water and so adjusted in a frame that the observer could close the circuit by putting one finger of the same hand into each tumbler. The tumblers were filled with tepid water and each one was supplied with a rubber support, which allowed the fingers to be immersed 1^{cm} in the water.

By eight or ten trials I first found the threshold at which he could perceive the stimulus, i. e. the ordinary tickling sensation produced by the interrupted current, then I left it stationary so far above the threshold that the current must necessarily be perceptible during all ordinary physical and mental fluctuations. The stop-watch was so connected with a rocking commutator in the secondary circuit, that when the commutator closed the circuit and thereby applied the stimulus, it simultaneously started the stop-watch. The observer stopped the watch with his reaction.

He was seated behind a screen and could not see the watch and the commutator, but could hear the former faintly. The instruction was that he should associate the tick of the watch with the electrical

stimulation. He was asked to react when he first perceived the sensation produced by the current, after the warning "Ready."

To limit the suggestion I made the interval between the warning and the application of the stimulus very irregular. The observer began to hear the tick of the watch the same instant he felt the shock from the stimulus. This was repeated ten times, and after that the stimulus was occasionally omitted but the warning given and the watch started. When nothing was said about the watch, the hallucination very seldom failed to appear, but when the observer was positively warned not to let himself be influenced by the sound the hallucinations were less frequent. When the watch was not used and the warning was still given regularly in the preparatory trials, only a small per cent. of the observers were hallucinated.

In this test, as in the first test on taste, fear or excitement may have been the immediate cause of the hallucination for some observers. In that respect these tests are exceptional.

Though I have cited no measurements on this class of hallucinations I have the experimental evidences, which might have been thrown into tables here had it not been superfluous, after the reports on sight and sound.

Conclusions.

From this series of experiments the following conclusions may be drawn.

1. Hallucinatory sensations of liminal intensities of touch, taste, smell and electric shocks may be experimentally produced in normal life.

2. According to the methods here employed, these hallucinations were produced, (1) by leading the observer to expect the respective sensations at a certain time and in a definite manner; (2) by associating the desired sensations with a warning; and (3) by associating them with simultaneous and continuous stimulations of other senses.

General remarks on the experiments in Part Second.

The general principle here studied belongs to the realm of the influence of feeling in perception ; but this series of experiments is limited to the influence of the more intellectual emotions, and these are reduced to a minimum as in the process of ***deliberate, voluntary, expectant attention in normal unexcited, intelligent persons.***

Many faults may be found with the above pieces of apparatus from the point of view of physics, for none of them afford means of absolute control and measurement of the physical stimulus. I cannot say I produced so loud a sound or so strong a taste, measured in absolute physical quantities, assuming a parallel between the physical stimulus and psychical correlate ; but the main desideratum was to secure such conditions that the observer could say without hesitation, *I see*, *hear*, *touch*, *taste*, etc.

Since the method was a combination of the experimental and the statistical, it was impossible to secure trained psychologists as observers in all cases. Nor was that necessary, for if I obtained observers with a good general power of discrimination and reliable judgment, the conditions from the experimental side were fairly satisfied ; and, by taking a large number of persons, the demands from the statistical side were not neglected.

The tests purport to be made in *normal*, *waking* life. With this condition in view much discretion has been exercised in selecting such observers as could most properly be spoken of as normal. Advanced students (men) would naturally be considered the most reliable observers ; at the same time they were the most easily accessible to me and readily interested in the work. I always tried to keep the observer uninformed as to the actual outcome of the experiment.

All possible precaution was taken to secure the conditions of a normal judgment. The observations were made under the most favorable physical conditions ; distraction was guarded against ; automatic reactions were practically precluded ; and the case was in general made so simple that the important conditions could be fairly controlled.

The psychological process involved in these experiments is expressed by one general principle—suggestion.

Suggestion presupposes mind as a selective agency which has the power of choice and interpretation, and that this choice and manner of interpretation can be directed to a great extent by external stimuli and central associations.

We may outline the characteristics of its use in these experiments. The method was to induce expectant attention to a liminal sensation by the presence of an object or some circumstance connected with it, in that way awakening a mental image which should realize itself in a sensation. The first step was to give the observer an idea of what to expect. His attention was called to the apparatus and the prin-

ciple by which it produced or served as a stimulus, and an actual example of the character of the stimulation was produced. Hence he acquired an image of the given sensation, and an idea of the process by which it was produced and began to associate them as cause and effect. It was next necessary that he should suggest to himself the fact, manner, and time of the recurrence of the sensation. The methods were such that he would build up this suggestion by a series of associations which may be reduced to the following four classes.

1. The observer was by his own choice and interest led to notice the manipulations of the apparatus and form a definite association between the action of the apparatus (or his change of relation to it) and the resulting stimulation, so that he knew *what* the result would be, *that* he would perceive it, and *when* he must expect it, e. g. in the experiment with the heated wire and with the bead.

2. A signal or warning became associated with stimulation in certain qualities and in time succession, e. g. in the light and the sound experiments. The observer suggested to himself certain permanent relations between the signal and the sensation, without having been told anything to that effect. The signal aroused the mental image and in the absence of any inhibition it realized itself in the customary time and manner.

3. When the apparatus was not in view and no signal was used, a rhythmic order of the recurrence of the sensation was sometimes set up, e. g. in the hallucinations of the tick of the clock and some of the tactual hallucinations. The physiological tendency toward rhythmic action and the mental associations of regular recurrence established a firm auto-suggestion upon false grounds.

4. By synæsthesia one sensation brought forth another sensation. A certain stimulation produced not only the corresponding sensation but also associated sensations of the same sense or of entirely different senses. This is more or less included in the three preceding methods, but it is particularly involved in the test on electric stimulation and some touch and taste experiments.

Expectant attention is the unitary principle in all these kinds of suggestion. By it a lifelike image of the sensation was awakened and the judgment of the observer was so profoundly influenced by this that he convinced himself of the actuality of the sensation for which no corresponding physical stimulus existed.

PART THIRD.

Some conclusions and deductions.

The following statements are deductions that seem to be justified by the experimental data and the facts established by them.

Experimental.

Judgments, which were the phenomena measured in this investigation, do not depend alone on the particular sensations judged, but also on other sensations and on interest, imagination, and expectation, in which feeling predominates ; attention and motor readiness, where will predominates ; and memory, discrimination, and association, where thought processes predominate. Many of the associations and states of mental preparedness depend upon suggestions from the environment. A seemingly insignificant word, thing, or circumstance may determine what the observer shall perceive or not perceive.

The effect of disappointed expectant attention in judgment of weight is shown in Part First ; and it is reasonable to suppose that it enters into other classes of precepts in a similar manner. In Part Second, the effect of expectant attention which frequently realizes its object is set forth. In most psychological experiments it is necessary to secure close attention. It is well known that by inattention an endless number of errors occurs, but it is often overlooked that forced attention, which necessarily rises into expectation, is one of the factors that must be carefully guarded against in experiments on liminal differences. An inference which I consider legitimate in regard to all these results is, that if there is so great deception in perception of small differences, there is a corresponding deception in regard to details in perception of large objects or groups of objects.

The experiments in Part Second demonstrate four different methods in which this vitiating effect of expectation may enter.

1. Attention to the associations between a sensation and a definite process of physical stimulation may cause an illusory interpretation of the sensation or cause the sensation to occur without the physiological modification by the external stimulus. The experiments on hallucinations of temperature, e. g. prove that our interpretation of liminal sensations is utterly unreliable, when we attend to the physical stimulus by other senses than the one stimulated. Hence if we would have a sensory perception true to that normally perceived

from the external object, special precaution must be taken in this respect.

2. Attention may rise into a firm expectation by means of a definite signal. Psychologists have recognized that when a mental image of a sensation has been produced and a signal given, the time of the perception will be influenced even to so great an extent that so-called negative reactions may occur. The above experiments have proved that the use of a signal in experiments upon liminal sensations has a controlling effect upon the sensation to follow; in view of these results it is necessary to reconsider the use of signals in the customary way.

3. In our ordinary physical and mental activity there is some degree of rhythm or time-order. Even if we guard against associations with apparatus or warning, there is yet a trap. If successive trials are given in some order so that the observer can in any way have reason to expect the sensation at a certain time, the discrimination will be influenced. This applies even if it be required to discriminate between two definite sensations, either of which may appear. Also, if two sets of experiments with a slight difference in the rate of change, e. g. be taken immediately after each other, the expectation based upon the experience in the first set will influence the perception in the second set. Hence the importance of arranging single trials, as well as sets of experiments, in the proper order so as to eliminate the effect of expectation.

4. By virtue of synæsthesia, we often perceive single qualities by two or more senses. One sense suggests to the other either by aiding in completing the image, as sight and touch, or by associating sensations which generally occur together in consciousness, as in combinations of sight and sound, in which there are inferences from one to the other. Therefore it is important in laboratory experiments to control the stimulation not only of the sense which is experimented upon, but also the stimulations of those senses which may in some way be associated with it in experience, otherwise the observer may easily deceive himself by taking the stimulation of one sense for the stimulation of another and actually perceive the latter. This kind of suggestion is more or less involved in all the preceding classes.

Pathological.

1. Illusions and hallucinations and all the phenomena in which they are the constituent elements (except those due to physical

pathology) may be experimentally produced in mild forms in fairly normal states, for the purpose of studying their nature.

2. To be normal is to have a firm inhibiting force in the form of discriminative consciousness which checks the realization of vagaries of imagination.

3. Experiences in all forms of illusion may be realistic. People really see ghosts. If a scientific observer in the bead experiment sees the bead as real although there is no bead, I do not think we can set any limit to what an excited, imaginative person may really see under circumstances favorable for illusion. Being fully convinced that the sensations produced by suggestion in my class of observers were, as a rule, real, we may safely infer that the suggested experiences during hypnosis are as a rule realistic. Arguing by analogy from the present experiments, we may go far to explain all the phenomena which depend upon sense illusion. The modern sleight-of-hand performer does not pretend to do anything but delude his audience. How much of the spiritualistic seance and all the phenomena of that category would remain, if a psychological analysis of the illusions were pushed far enough? Yet good men and women tell us those wonderful experiences are real.

4. Mind acts according to laws in normal states, and abnormal states are caused by a morbid or overstimulated activity of the same laws when some side of the regulative activity of mind is dormant.

5. GREISINGER[1] assigns the following causes for hallucinations: (1) local disease of organs of sense, (2) state of deep exhaustion either of mind or body, (3) morbid emotional states, (4) outward calm and stillness between sleep and waking, and (5) the action of certain poisons. The experiments I have made justify the addition of another general source of illusion, namely, expectant attention.

The different forms of suggestion in which this source of illusion is prominent, as determined in the experiments, corresponds to the four kinds of associations which constituted the suggestion as detailed in the foregoing section. Thus, the association of a sensation with the physical stimulus which is supposed to produce it constitutes the virtue of all the devices which have been used to induce hypnotism,—magnets, vials, gongs, crystals, fountains, incantations, and even the word and presence of the hypnotizer. They all serve the same purpose and have the same virtue, i. e. they are the means by which the subject arrives at a certain degree of expectation and conviction.

[1] SULLY, *Illusions*, p. 115.

Epistemological.

A thorough investigation of the epistemological problem begins with a study of the psychology of perception, the simplest form of immediate knowledge. The first question there answered will naturally be, "To what extent is our sense-intuition reliable?" This part of epistemology should be treated in the psychological laboratory. Extended laboratory experiments with a thorough introspective analysis must pronounce upon the psychological problem of sensory illusions and hallucinations, and metaphysics must take this decision into account.

The above results are of a very rudimentary character with reference to this problem, and they were not made with it especially in view, but to me certain empirical facts have been made prominent and clear.

Looking first at the side which disparages our confidence in knowledge of things as they really are, the following considerations present themselves. All mental activities are involved in common acts of perception. The intricate process involved in the perception of a single quality was illustrated by the weight test, and the same line of observation might be infinitely extended to show that to become known to me the quality of an object must enter and modify my stream of consciousness and adapt itself to it. In this complex process the data of sense are profoundly modified by central states and activities. We noticed the influence of one of these, expectant attention, in its twofold character as disappointed expectation and as realized expectation, when based upon false grounds. What we call normal perception involves many illusory influences—not only those of physical and physiological origin, but even more so those due to the functions of ideation, memory, and imagination. Indeed, suggestion and imagination control all our perception by the senses. Very intelligent men are liable to embellish and misinterpret their sense data even under circumstances favorable for accurate perception. The perception of liminal differences is subject to so many misleading influences that, as a rule, it is extremely unreliable; and the cognitive functions act very imperfectly in giving us a detailed representation of external objects.

But on the positive side, confirming us in the belief that somehow we perceive things as they are, several important facts may be observed. Illusions work according to laws which may generally be determined. As these become known we may gradually learn to

rule out the illusion. The known physical and physiological illusions do not necessarily delude us because we may make allowance for them. Similarly, we may now make an approximate allowance for the illusions of weight and for all other illusions, due to intellectualized feelings, as they become recognized. The view that illusions and hallucinations do not act according to law is as wrong as the view that mind in its normal capacity is lawless. The more thoroughly we become acquainted with the laws of illusions the more accurately will our sense perceptions fall in consensus.

STUDIES OF FATIGUE.

BY

JOHN M. MOORE, PH.D.

EFFECT OF FATIGUE ON BINOCULAR ESTIMATE OF DEPTH.

Early experiments.

The first apparatus consisted of a board about 1½ meters in length into which were driven two knitting needles exactly one meter apart. Between the needles was a brass rod, supported by two small blocks of wood, on which was a sliding carriage with a needle driven into its center. A bead was placed on each of the needles. The beads were adjusted in a straight line and at the same height. The head of the observer was put into a support which kept the eyes in line and at the same height with the beads. He was told to look at the bead nearest him, then at the next, and then at the last. He was to judge of the equality between the distance from the first to the second and that from second to third. If the second bead was too near or too far away he moved it by a cord till the distances were equal.

Experiments were made for several days upon myself and others. On looking at the beads six images were seen. The nearest pair were first united, then the second pair and finally the third pair. There were thus never less than five images present, which changed in position with every adjustment. The effort to combine the proper images was constantly hindered by the distracting movements of the others. These double images interfered so much that in my own case I was unable to get more than twenty results at one sitting, on account of great fatigue.

Each experiment gave a record of the distance of the middle bead from the nearer one. Taking the position of the bead in the first experiment as the standard, its record was subtracted from each following record. The results thus obtained show how much the middle bead recedes from the nearer one as fatigue comes on.

Thus three sets of experiments were made whose results were transformed into "fatigue sets" by subtracting the first result in

each set from each of the following results in the same set. The averages of the three sets gave the results, 0, 12, 29, 35, 30, 50, 60, 85, 98, 95, 113, 116 in millimeters as the amount of increase over the first. The amount of error due to fatigue during these twelve experiments was equal to about the amount of error for 40 experiments according to the later method.

With others experimented upon, the double images likewise distracted the attention and hindered a satisfactory judgment. The observers never felt sure of the distance from one bead to the other, as their attention was required to adjust the double images.

This was not the only objection to having the three beads before the eyes at one time. When the eyes were converged on the first, with the second and third double, there was formed a triangle of which the first bead was the vertex while the two images of the third were at the base. With this outline before the eyes, the second bead could easily be moved until its double images became the centers of the sides of the triangle. This could all be done without any movement of the eyes.

Final apparatus.

A board, fig. 7, a little more than one and a half meters long and thirty centimeters wide, supported by three legs, served as the table.

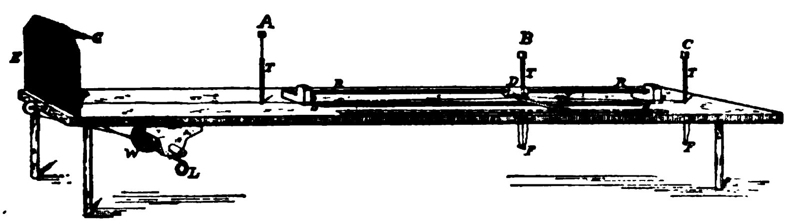

Fig. 7.

From the end at which the observer sat, at a distance of one-half a meter, the first brass tube T was stationed, extending down through the board. In this tube was a knitting needle, on the upper end of which a cylindrical brass bead 10^{mm} long and 9^{mm} in diameter was fastened. On the lower end of the needle a small rod was fastened. The needle had an up-and-down play of 45^{mm}. Another bead C was similarly supported at a distance of 1^{m} from the first. A slot S, 70^{cm} long, was cut in the board between the two beads. Over the

center of the slot was a brass rod R, 1^{cm} in diameter, supported at either end by small blocks 2^{cm} high. Upon this rod was a small brass cylinder D, closely fitting but free to move, which acted as a carriage for the second bead B supported similarly to the other two. It carried an index I which extended over a scale graduated in both directions from the center of the distance between the two beads. This carriage was moved by an endless cord passing over pulleys at the ends of the board. It also passed over a wheel W, whose axle was supported by two blocks under the table at a short distance from the front end. By turning the protruding axle the observer could place the middle bead wherever he desired. By pushing the lever L the first bead A could be raised and by pulling it the third bead C could be raised. This was done with the same hand which turned the roller for the second bead. From the rod F on the lower end of the second bead, a cord passed up behind and over the index rod I of the carriage, then through the slot S, under the table to a screw-eye in the front. This was used by the left hand and was kept stretched by a small weight.

The beads were hidden from the observer by a blackened upright board E. In this a slot 2^{mm} wide and 11^{cm} long was cut, at the height which the beads would have when raised. To prevent the eyes from looking down further on the third bead than on the other two, by virtue of the visual angle covering more distance, a black tin apron G was extended 15^{cm} out from the slot. The head was kept in the same position by putting the nose at a definite place each time.

The apparatus was placed on a large table and was raised or lowered so that when the observer was sitting erect, the eyes would be opposite the slot. The beads when raised were in the direct line of vision from the center of the slot. The nose having been put upon this middle point, each bead formed the vertex of an isosceles triangle made by the line of sight of the two eyes. No movements were necessary except those of accommodation and of symmetrical convergence.

Method of experiment.

The experiment began each time with the carriage of the second bead placed at the end of the slot nearest the observer. All beads were out of sight. By a push of the lever the first bead was raised, its position was noticed and it was allowed to fall. By the cord in the left hand the second bead was raised and dropped. Then by a

pull of the lever the third was raised and dropped. The judgment was made as to whether or not the second bead was in the middle. If not, the block was moved further away by turning the wheel and the experiment was repeated. This was done until the bead was judged to be in the middle.

A few trial experiments showed that almost the same results would be obtained if the experiment began with the second bead at the near or at the far end; but as my judgments always placed the bead nearer the farther end, it was best to bring the slide over the greater distance to prevent the judgment from being influenced by the distance moved. Had it been moved otherwise the distance was short enough to have been remembered. Care was always taken to prevent the judgment from being influenced by the distance passed over by the carriage, or by the amount of cord passed through the hand, or by the number of revolutions of the wheel.

The distance of the first bead from the eyes was a half meter; this is not short enough to cause any strain. The third bead was a meter and a half away and the middle point was 500^{mm} from either extreme. The part of the scale graded from this point toward the eyes was called negative and the other positive; they were indicated by the signs — and +.

The experiment was never continued to complete exhaustion. In the first experiments on myself, I continued until vision became very indistinct and the third bead was focused with great difficulty. This occurred about the fortieth experiment. Thereafter the set of experiments was ended at the fortieth.

Each individual experiment resulted in a position of the middle bead somewhere near the middle point, or 0, between the two end-beads. Its divergence from this point was recorded in millimeters + or —.

Observer A.

The first question to be considered is the position of the estimated middle point for the first experiment on the successive days of work.

The results are: during May, 27, 60, 33, 28, 74, 97, 83, 96; during June, 75, 48, 48, 81, 80, 80, 90, 74, 61, 76, 86; during July, 92, 97, 102, 92, 103, 121, 136, 126, 70, 104 millimeters, all being + or deviations beyond the true middle. In all the experimenting upon myself the second bead was never brought on the minus side and the nearest plus was 37^{mm}. Thus in my normal condition the distance between the two beads was steadily overestimated.

This change of estimation can be attributed directly to fatigue for the following reason. When I began experimenting on myself in May, I was in excellent physical condition and my other work was not heavy. Much time was afterwards required for outside work and the pressure became so great that I could go to the laboratory only after the most exhausting labor. During the last few days of the experimenting, a feeling of general physical depression was experienced even at the beginning of the experiment. Further experimenting was postponed till June.

The June experiments were begun at the end of a month's work of the most exhausting character. The outside work had now narrowed to merely an employment which was not at all fatiguing but which prevented any rest. The wear of the previous severe strain could not be easily repaired. There was very little change in the physical condition and the record shows very slight variation.

TABLE I.

Fatigue in binocular estimate of depth.

No. of record	1	2	3	4	5	6	7	8	9	10	11	12	13
May, average	0	3	9	20	17	28	19	21	30	47	43	48	62
June, average	0	2	4	6	13	18	10	18	18	20	37	41	33
May and June, average	0	2	6	12	15	22	14	19	23	32	40	44	45
No. of record	14	15	16	17	18	19	20	21	22	23	24	25	26
May, average	54	61	64	71	77	83	79	62	60	76	83	75	84
June, average	31	31	41	36	42	48	44	44	48	49	48	51	44
May and June, average	41	44	51	50	56	63	58	52	53	60	63	61	61
No. of record	27	28	29	30	31	32	33	34	35	36	37	38	39	40
May, average	92	78	81	87	96	91	104	88	82	109	86	99	91	105
June, average	41	43	49	58	54	52	61	60	58	66	56	65	62	65
May and June, average	62	58	62	70	71	67	78	71	67	72	68	78	74	80

During July the work was steady and experiments on the eyes were made two or three times each day till Aug. 1. The eyes were becoming greatly fatigued. The exceptional character of the ninth experiment in July is due to the fact that Sunday intervened between the eighth and ninth with a general rest of forty-four hours. The records for July show a large increase over the records similarly taken in May and June. The painful sensations in the eyes testified to fatigue. In the beginning of the experiment, at the first convergence, a severe pain was often felt over the eyes. When any near object was focused the same painful feeling was experienced.

A suggestion is here offered to the teachers in primary and sec-

ondary schools. Where proper supplies cannot be furnished by the community, a large amount of copying from a blackboard is required. Small children are frequently compelled to do this work, although the fatigue is particularly injurious in their case. They are not able

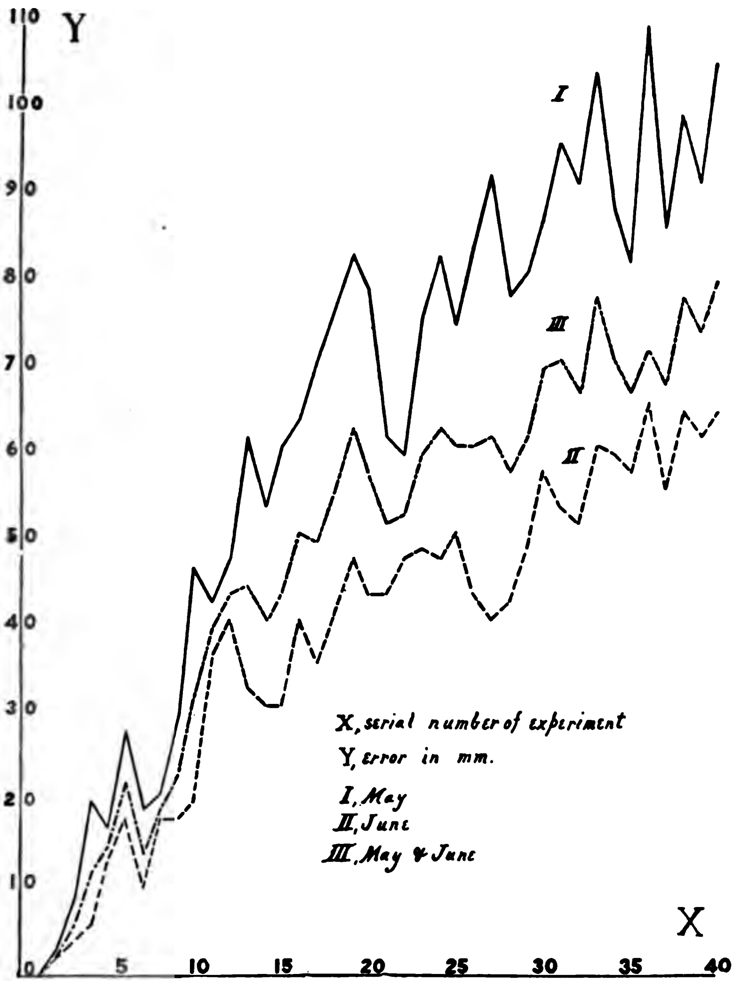

Fig. 8.

to grasp a sentence and write it from one glance; they must continually change their convergence by looking to the board and back. Even larger pupils are very often required to copy Latin, Greek or German from the board. These languages not being familiar, the

convergence must be changed for almost every word. If such work is continued many minutes in succession, or for a short time every day, the evil effect will be seen in those pupils whose eyes are not strong. They will be afflicted with headaches and will be greatly disturbed in the preparation of lessons.

The second question for consideration is the effect of the special fatigue in each experiment of the set of forty. In the previous paragraphs only the first experiment of the set was considered. With this first experiment taken as a standard, any deviation of the succeeding results from the same figure can be attributed to fatigue resulting from the previous experiments of the same set. Accordingly for each set the difference between each experiment and the first experiment was computed; thus, +3 for the second experiment would indicate that the middle point was placed 3^{mm} further from the eyes than in the first experiment. The result of the first experiment was thus used as a zero-point from which to reckon the effect of fatigue in the following experiments.

The average divergence of the second result from the first was +3 for the eight sets in May, that of the third was +9, etc. The complete list is shown in Table I, all the results being +.

The table and the curve, fig. 8, show an increase from the very beginning. In the curve for May, in the second experiment there is an increase of 3^{mm} over the first, in the third 9^{mm}, and in the fourth 20^{mm}, and so on. In the curve for June, 2^{mm}, 4^{mm}, and 6^{mm} are the increases of the second, third, and fourth experiments. In the composite curve for May and June the increases are 2^{mm}, 6^{mm}, 12^{mm}. From this it seems evident that fatigue begins very early in the experiment. As each single experiment represents, on the average, four distinct trials at accommodation and convergence it is safe to say that fatigue sets in within ten trials, though it may not be detected by any painful sensations for more than twice that number.

The increase for May is much more rapid than that for June. This is true for all the records. May ends with an increase of 105^{mm}, while June reaches only to 65^{mm}. While this may be attributed in part to external circumstances or physiological conditions, it is due to a large extent to practice, as has been pointed out by FECHNER.[1] Continued practice would enable the subject to make many more experiments before exhaustion than were made in this series. The form of the curve would continually vary until practice had reached

[1] FECHNER, *Ueber den Gang der Muskelübung*, Ber. d. k.-sachs. Ges. d. Wiss., math.-phys. Kl., 1857 IX 113.

its maximum. The general form would not be so much changed, but certain parts of the curve would be stretched out as in the case of those who are not easily fatigued.

The composite curve shows that fatigue changes our estimations very rapidly in the early experiments, while the successive increments decrease toward the end of the set. The curve rises more rapidly in the first half than in the last. The change in the last quarter is not nearly so marked as in the first quarter. Could the experiments be continued to complete exhaustion, the curve would assume a parabolic form. That is, the observer will continue to increase the overestimation until the bead reaches a certain point, beyond which it cannot be moved without the observer, however fatigued, knowing that it is nearer the third than the first bead.

The presence of rhythm is very marked. It disappears somewhat in the composite curves, but it is very striking in the individual records. There seems to be a rhythm of fatigue throughout the whole work. There are exceptions but they are few. Rhythm has been found in all the work in which fatigue comes in to modify the results. The following record of a single experiment shows the phenomenon. The figures indicate the number of millimeters of overestimation at each placing.

It runs 81, 75, 42 (which is exceptional as they generally rise from the beginning), 74, 104, 120, 54, 85, 87, 97, 119, falls to 102, but rises immediately to 134. Some parts of the remainder show similar fluctuations: 105, 100, 137, 138, 125, 129, 137, 144, 141, 115, 125, 150, 146, 114, 164, 155, 150, 138, 186, 142, 159, 123, 172, 171, 133, 148, 199. This fluctuation reminds us of LOMBARD's experiments on muscular fatigue.[1]

The figures in Table I are averages for experiments extending over a whole month. Thus, the number 105 for the 40th experiment is the average of all the 40th experiments for the month of May. It becomes necessary to know how the individual results differ from the average. This knowledge is obtained from the mean variation. Each individual result used in an average had its difference from that average determined; thus, each 40th experiment during May was compared with 105 and the difference noted. Then these differences were averaged, giving the mean variation; thus a mean variation was obtained for the 40th experiment, and likewise for each of the others. The results are given in Table II.

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[1] LOMBARD, *The effect of fatigue on voluntary muscular contractions*, Am. Jour. Psych., 1890 III 24.

The table and the curve, fig. 9, show the same fact as the fatigue curve itself, although not so marked. The curve for May begins with eight and rises to forty-one. The curve for June begins with sixteen and does not reach higher than twenty-nine, and then closes near its beginning. In the fatigue curve for May there is a much larger increase than in that for June. As fatigue increases work becomes more irregular. This will be found also in the results of the mental and muscular work which will be described later.

TABLE II.

Mean variation in binocular estimate of depth.

No. of exper.	1	2	3	4	5	6	7	8	9	10	11	12	13
May	0	8	19	14	19	21	17	16	26	21	16	25	22
June	0	16	15	15	8	22	16	17	14	16	14	19	22
May and June	0	13	17	16	12	12	17	17	20	23	15	22	27
No. of exper.	14	15	16	17	18	19	20	21	22	23	24	25	26
May	12	26	24	34	30	36	41	23	26	33	29	27	27
June	12	20	16	15	13	13	19	19	16	19	15	19	18
May and June	16	25	21	27	24	26	33	21	21	27	25	25	16
No. of exper.	27	28	29	30	31	32	33	34	35	36	37	38	39	40
May	33	26	34	34	34	36	29	36	24	41	30	35	40	29
June	22	23	29	18	19	20	18	29	24	23	22	19	19	17
May and June	29	26	32	27	31	27	27	32	26	35	27	30	32	26

What is the source of this irregularity? By an electrical stimulus it might be proven that the fatigue was central, as was found by Mosso[1] in muscular fatigue. Fatigue in the physical organism might cause an extension or contraction of the estimated distance, but it could not explain the irregular element. This must be accounted for from a purely psychical standpoint. The will, attention, and memory were involved in every decision. When fatigue had fully begun, it required considerable effort to hold the attention on the position of the beads and to retain in mind the two distances to be compared. Often it became impossible to remember with clearness the first distance, and unless the decision was made immediately, only two dim perceptions, one of which was fast escaping, were present. The fatigue evidently affects the attention and thus causes this irregularity.

During the first week of July six sets of experiments were made

[1] MOSSO, *Ueber die Gesetze der Ermüdung*, Du Bois-Reymonds Arch. f. Physiol., 1890 111.

to determine the relation which memory bears to the results. The subject would focus on the first bead, then the second, and then wait five seconds, and again focus the second, and then the third.

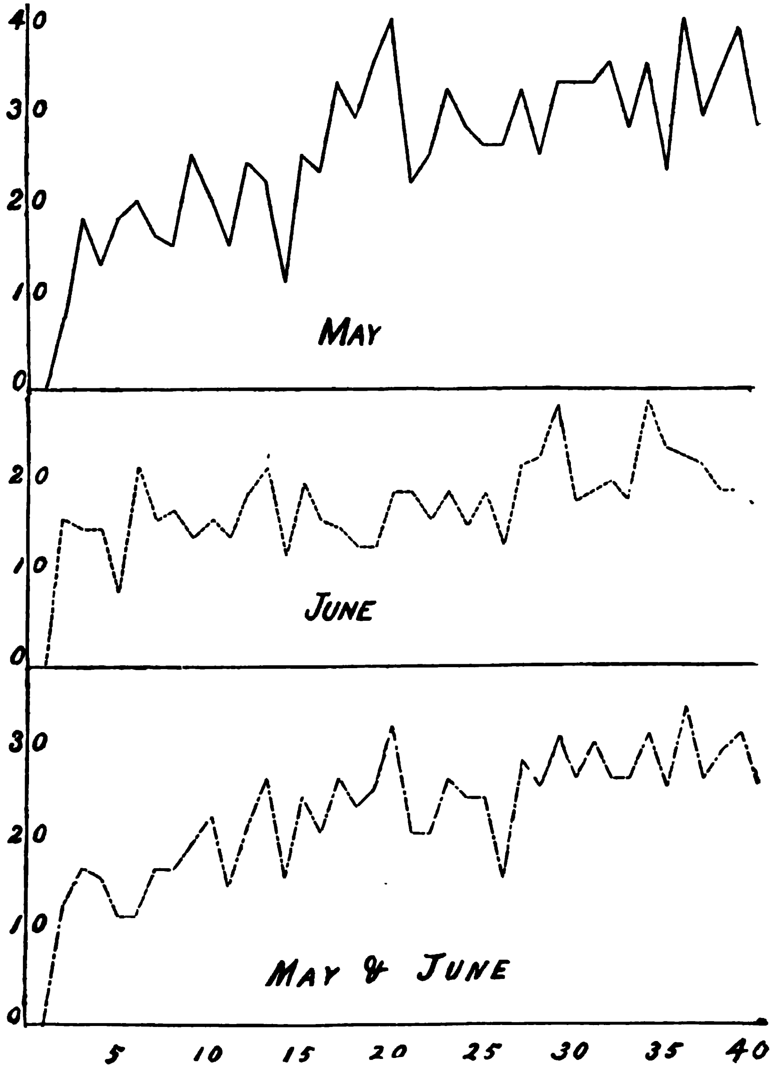

Fig. 9.

The result is seen in the following average record of the fatigue : 0,– 6, 13, 11, 7, 6, 13, 13, 20, 21, 21, 18, 28, 19, 60, 23, 39, 36, 57, 40, 49, 38, 63, 53, 56, 54, 61, 58, 53, 42, 43, 62, 53, 63, 65, 57, 52, 50, 67,

59^{mm}. The effect is found in the greater irregularity. As in the preceding case, the increase is produced but according to no law. About the same result was found when the time was extended to ten records.

Why should this exhaustion come so early? Why should a fatigue of 52^{mm} appear in the twentieth placing while in the regular work it was not reached before the fortieth? The explanation is perhaps as follows. When the subject raised the first bead and then the second, he formed a mental picture of the distance. He waited five seconds and raised the third bead. Now, two positions are held in memory and the third is before him. The two memory-positions will approach each other as the time passes, and the second bead will appear nearer than it did when it was focused. The subject is not conscious of this mental operation. So he pushes the second bead farther from him than he would have done had no time elapsed. The first placing shows this phenomenon. As fatigue comes on, our memory images grow dim much faster and consequently we increase the distance very rapidly. Fatigue has the same effect on memory distance as the passing of time. Why exhaustion comes so early will be spoken of later.

Observer B.

This observer was a college student. A large number of trial experiments was made before his records assumed a regular form. His irregularity was due to his manner of focusing. His first efforts were made, as he said, in trying to find the middle point, and as a result he invalidated his work by following several different methods. Finally, after much insisting that he locate each bead mentally by looking at it and at nothing else, his curves began to take a more regular form. However, his mean variation continued large. This seems to indicate an inferior accuracy in judging depth by binocular movements. The large mean variation doubtless covered in the record much of the misplacement that was due to fatigue.

Out of his numerous records twenty consecutive ones were selected. In every case the second bead was placed nearer the third bead than the first. The averages of the twenty fatigue sets made as in preceding work is as follows: 0, 3, 7, 2, 11, 6, 18, 17, 15, 22, 29, 30, 25, 27, 29, 31, 31, 24, 32, 33, 34, 33, 33, 32, 32, 45, 31, 26, 25, 29, 31, 27, 36, 28, 34, 26, 33, 31, 29, 34, 27. As in the previous case, each number represents the amount of retreat of the bead in the successive

experiments beyond its place at the first experiment. The curve of results is shown in fig. 10.

His fatigue curve does not reach the height of that for Observer A, although he testifies to fatigue in these notes: "eyes ache;" "eyes feel tired;" "extremely tired, eyes worn out;" "eyes felt heavy and tired; it was hard to focus them." The only explanation of the difference in the records seems to be that the average of the first records in all the experiments on Observer A is 72, while the average of his is 175, which is near A's fatigue limit. His fatigue range

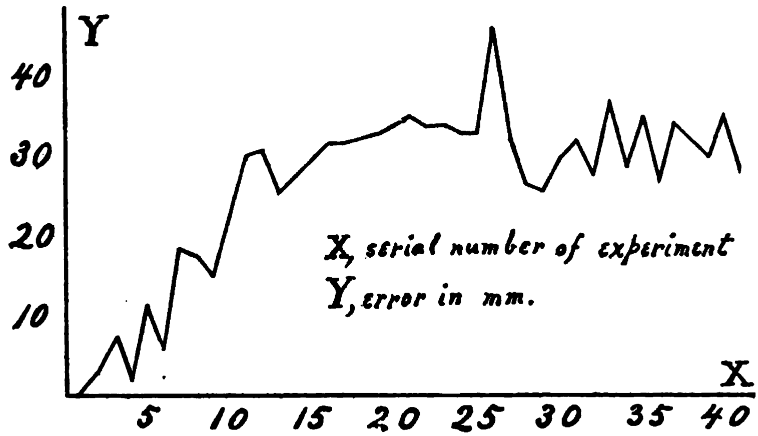

Fig. 10.

could not extend so far without bringing the second bead near the third. Fatigue would not produce so great a change within that small space. With a longer distance the curves would be likely to come nearer each other.

B's greatest fatigue was generally reached several experiments before the set closed. In the average curve the highest point is at the twenty-fifth experiment. This is true of the majority of the individual curves. At this point he would generally complain of being "unable to go much further." He would soon feel better and his record would show a less distance from the center than in the immediately preceding experiment.

Fluctuations were present as in the case of Observer A.

Practice had approached its maximum influence before his records were ended. The time required to place the bead was much longer than for other observers. It required twenty minutes for A to make the set of forty, while it required thirty minutes for B. He was

slow in decision and would often focus twice or more without moving the beads. This extra focusing may account for the fact of his reaching a maximum fatigue so early as the 25 experiment.

The mean variations for his fatigue curves show a slight increase for successive dates. His irregularity in the individual records was much greater than that of the preceding observer.

Discussion.

What was the condition of the eyes while one bead was being lowered and the other raised? Did they hold their convergence or did they assume a parallel? To determine this, a fourth bead was set up just one meter from the third. The first bead was raised, noticed, and then dropped. The eyes were then focused on the fourth. The second was raised and dropped, and the eyes were again focused on the fourth. After the same action with the third, the judgment was made as to whether or not the second bead was in the middle. At the twentieth experiment the eyes were exhausted, while they were accustomed to forty experiments with the three beads. The attention was so distracted that it was difficult to place the second bead. The time required for focusing was largely increased. The same results were found when a mark on a distant wall was used instead of the fourth bead. The observations show that the eyes rarely assume a definite intentional position, but rather an easy position from which the next converging can be done with the least difficulty. This position is presumably a parallel one.

In making these experiments by putting up a fourth bead to determine whether or not the eyes remained converged between the focusings, the attention was greatly distracted. So, to find the effect that the fatigue of attention had on the estimate of distance, this bead, marked by the letter D, was placed at a distance of one meter from the third and in the same line of vision. Ten sets of experiments were made as just described, the average of whose fatigue sets is as follows: 0, 0,–2, 7, 13, 13, 16, 24, 22, 20, 30, 44, 32, 33, 50, 47, 52, 54, 60, 54. The highest effect of fatigue is 60^{mm}, which is about the same as that found in the regular sets. But this is the result of only twenty placings, the regular sets containing forty. The time required to make these twenty experiments was eighteen minutes while the time of the forty was only twenty. In these the firmest attention was absolutely necessary to judge the distance and hold in mind the position of the beads. When the first was focused, then the fourth, the second, then the fourth again, the distance from the first to the

second was forgotten unless the memory was kept riveted on the various positions. The fourth greatly interfered by distracting the attention. As fatigue entered, the work grew more difficult, the attention was more easily distracted, and as a result the variation became much larger in the individual experiments. The muscles of convergence could not have been very much more fatigued by extra fixation. The additional fatigue must have been due to the strain on the attention.

Comparison with atmospheric changes.

As the season was early summer, the windows were generally open and the conditions within the room were about the same as without. The observer at the United States Weather Station, which is about 300 meters from the laboratory, furnished me with the temperature, humidity and barometer markings. These fairly represented the conditions of the room used.

In Table III the fatigue column was found by subtracting the average of the first five of each set from the average of the last five. This was done for each experiment for the days in May and June and the results were placed in the order in which the experiments were made. The temperature and barometer readings were taken every hour. The humidity at the Station was registered only at 8 A. M. and 8 P. M. and so it was necessary to interpolate for the hour required. The humidity may not be correct, as it does not follow arithmetical rules.

By comparing the fatigue column of Table III with the columns representing the temperature, humidity, and barometer markings, it is at once seen that the fatigue was not controled by any one of these atmospheric conditions. On the first day, when the amount of the fatigue was the greatest, the barometer was exceedingly high. The second greatest fatigue has the second highest barometer. The second day shows an increase in temperature (but the temperature at that point would not be depressive), a slight decrease of humidity and a decided decrease in the pressure, while the fatigue is greatly reduced. On the third, the humidity and pressure decrease, the temperature increases and the fatigue increases. The first two are not sufficiently high to produce any evil effects, while a temperature of 84° so early in the summer in a northern climate will prove depressing. Throughout the experiments the law seems to hold true, that when temperature, humidity and atmospheric pressure are

free to act, the fatigue is accelerated or retarded by the combined increase of any two of the three influences. But a large increase of temperature with a small decrease of humidity and pressure will increase the fatigue. Fatigue will be increased by a heavy increase

TABLE III.

Comparison of fatigue with atmospheric conditions.

Fatigue.	Temperature.	Humidity.	Barometer in inches.
147	60	73	30.24
89	77	69	29.89
101	84	65	29.69
104	61	68	30.05
87	48	79	30.02
43	51	69	30.00
54	75	76	29.64
83	71	80	29.85
90	80	90	29.88
95	82	90	29.86
66	86	76	29.86
64	88	76	29.85
59	91	66	29.84
68	93	68	29.81
34	91	69	29.80
51	60	98	30.04
73	80	95	29.83
51	78	84	29.83
52	76	96	29.93

of any one of these against a small decrease of the other two. Also a large decrease of one will decrease fatigue against a small increase of the other two. If the subject were put into a room where two of these influences could be kept constant while the other could be varied, it would probably be found that an increase of temperature, humidity or pressure would tend to increase the amount of fatigue, provided, of course, the experiments were begun at what might be termed the normal point of each.

EFFECT OF FATIGUE ON MONOCULAR ESTIMATE OF DEPTH.

Perspective vision with one eye is generally imperfect, obscure and inaccurate. It has been held that the sensations of accommodation are not sufficient for any estimate of the depth of objects with one eye.[1] In the experiments here described all secondary helps were

[1] HILLEBRAND, *Das Verhältniss von Accommodation u. Konvergenz zur Tiefenlokalisation*, Zt. Psych. Phys. Sinn., 1890 VII 97.

eliminated as far as possible in order to cause the judgments to be made from the sensations of accommodation. The same apparatus was used that was used for determining with both eyes the effect of fatigue on the estimate of depth. The slot through which the observer looked was reduced to 3^{cm} in length, so as to permit the use of only one eye, the left, and to hold that in the same line of vision.

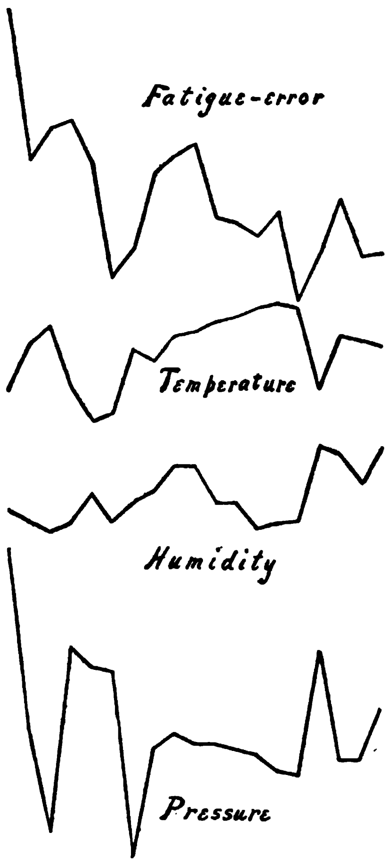

Fig. 11.

The other eye was not closed but left free. As it was within 3^{cm} of the black shield there was nothing on which to focus and the attention was given entirely to the seeing eye.

The experiments were made upon myself and Observer B during the month of July. At first there was great difficulty in accommodating or focusing. The beads would not become clear and sharp at

once. Several sets were made before the judgments could be relied on. The first record and the average of the first five records in each set are given in Table IV.

The results show an increasing tendency toward placing the middle bead beyond the middle. This is similar to the result for binocular vision and for nearly all fatigue sets.

The averages for fifteen fatigue sets taken on three observers are shown in Table V, the fatigue set being formed, as before, by subtracting the first record from each of the following ones.

The form of the curves in fig. 12 is not very unlike that found when both eyes were used. The greatest increase over the first experiment comes within the first twenty experiments. The curve after that time takes more nearly the parabolic form corresponding to that pointed out in the preceding section.

TABLE IV.

First record and average of first five records in monocular estimation.

No. of set	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Observer A, first record	62	72	50	47	80	26	46	73	137	83	135	72	68	114	123
Observer A, first five	81	81	63	57	96	39	60	79	133	82	128	93	63	96	115
Observer B, first record	2	5	122	53	107	112	125	150	135	137	136	175	205	171	
Observer B, first five	37	19	93	64	94	92	146	142	89	212	153	175	207	183	

The mean variation of the individual results from the general result is somewhat larger than that in binocular work. The effect of fatigue on the variation is not quite so marked, yet there is a larger variation toward the close than at the beginning. Fatigue affects the accuracy of the judgment. In the individual experiments the variation from one to the other is very much greater than that in the estimation with both eyes.

From the fifteen sets of Observer A, whose average has been given in Table V, the following sample record is given to show the individual peculiarities. It runs as follows: 26, 44, 46, 20, 59, 72, 78, 77, 119, 49, 54, 88, 39, 44, 56, 62, 92, 60, 110, 98, 54, 72, 132, 80, 104, 105, 88, 92, 78, 125, 80, 120, 146, 160, 126, 88, 131, 128, 141, 160. This is a fair representative of the series. The amount of variation from one to another is easily seen. The dropping from 119 to 49 was due likely to a mistaken judgment in the former. Such irregularity is to be expected much more than the wave motion, which is

to a large extent apparent. A gradual increase or continued variation would be more easily explained than the rhythm which nature seems to introduce. No cause for this phenomenon can be ascribed to any known external conditions. Some of the notes made were: "can scarcely see," this was when looking at objects after the experiment; "toward the last, the objects grew too dim," the power of convergence was greatly decreased; "focusing not clear, especially toward the last;" "hard to focus toward the last." As regards the right eye, which was not used, these notes were made:

TABLE V.

Fatigue in monocular estimate of depth.

Number of exper.	1	2	3	4	5	6	7	8	9	10
Observer A	0	−2	8	12	9	9	21	27	29	20
Observer B	0	1	−2	−7	−4	10	−20	1	12	13
Observer C	0	9	19	26	36	44	51	43	52	54
Number of exper.	11	12	13	14	15	16	17	18	19	20
Observer A	36	33	30	40	34	40	39	48	45	41
Observer B	−10	5	14	2	15	9	11	−3	−4	−3
Observer C	65	63	79	70	69	66	74	64	61	71
Number of exper.	21	22	23	24	25	26	27	28	29	30
Observer A	50	62	54	53	55	54	59	60	48	65
Observer B	−17	8	1	7	21	19	14	5	1	22
Observer C	69	67	71	76	85	61	75	76	80	80
Number of exper.	31	32	33	34	35	36	37	38	39	40
Observer A	52	50	66	59	57	53	71	67	67	70
Observer B	+7	14	−1	−1	4	6	23	4	1	−12
Observer C	91	74	75	81	87	92	89	83	99	89
Number of exper.	41	42	43	44	45	46	47	48	49	50
Observer C	103	104	82	89	92	103	106	107	98	104

The numbers are positive unless otherwise designated.

"right eye pained as much as the left;" "right eye felt worse till I got up, and then the left pained me very much." There is little doubt that the unused eye was accommodating and focusing with the seeing eye. It might be claimed that much assistance may have been received from its sensations of convergence. As its focus depended on the seeing eye, it is not clear how it could give any aid, although the claim that work with one eye is in reality nothing more nor less than work with both eyes is undoubtedly correct. The facts noted in this paragraph all refer to Observer A.

Table V also shows the relations of fifteen fatigue sets taken upon Observer B. The result is very different from that just pre-

sented. No law of fatigue can be discovered. The reason for this can be found by examining the individual record. One of them begins with 107 but falls to 81, then to 54. Then it rises and falls again. The second twenty minutes show a slight increase but

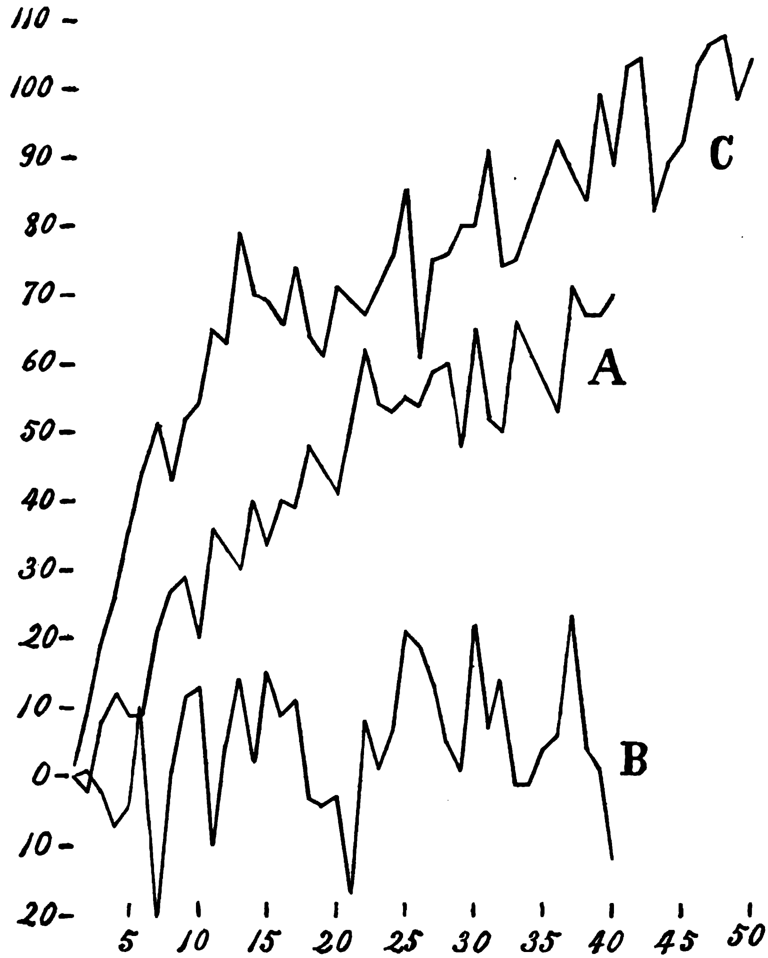

Fig. 12.

according to no law. This irregularity is found in all the fifteen sets. The averages apparently represent this observer as very accurate and unfatigued, but the enormous variations in the records show that this was simply due to counterbalancing errors in both directions. This was accounted for when the operator acknowledged

his inability to get a clear focus. He could not properly estimate depth with one eye.

To determine further the effect of fatigue on monocular estimation of depth, experiments were made on W. H. L., a young man who had lost his right eye. A few months after the removal of the eyeball, the wounds healed and his left eye became serviceable. At first he had great trouble in seeing objects near him. He could not fix his eye for the near objects. He could not estimate the distance from one object to another behind it. In looking from the third story of a building in which he was working the curb-stone seemed to be level with the street. This changed after a few months and he is now able to determine depth, according to his statement, almost as accurately as before he lost his eye.

He was free for experiment only at seven o'clock in the morning and at eight o'clock in the evening. After a few trials to acquaint himself with the use of the apparatus and to learn the exact nature of the experiment, he made seven records. From these the fatigue sets were formed whose averages are shown in Table V, Observer C.

The rhythmical movement is very manifest in all his records. With him fatigue increases the estimate of the distance between two objects lying in the same line of vision. The increase is not so rapid in the last as in the first part of the experiment. These conclusions agree with those obtained from the binocular work.

The accuracy of decision of this observer was somewhat surprising. Sometimes I manipulated the apparatus and moved it as he ordered. He could easily detect the "too far" or "too near" when the bead was moved more or less than he desired. His judgments were made entirely from what he saw and not from the noise of the slide or the amount of cord used.

The results here recorded conclusively show that fatigue of accommodation has an influence on localization in depth. If the conclusions of HILLEBRAND concerning the total failure of accommodation to aid a judgment of depth are to be accepted, as they apparently must be, it is very difficult to see any possible explanation for the fact that fatigue of accommodation influences such judgments.

EFFECT OF FATIGUE ON THE TIME OF MONOCULAR ACCOMMODATION.

In the Studies for 1893 SEASHORE[1] gives the result of researches made on monocular accommodation-time. On page 69 he says: "In

[1] SEASHORE, *On monocular accommodation-time*, Stud. Yale Psych. Lab., 1892–93 I 56.

regard to fatigue, however, my results are contrary to the usual supposition. Experiments of some 300 accommodations in one continuous set do not support that theory [increase of time]. Fatigue soon sets in and may become very painful, but as long as the eye can accommodate clearly it causes a fluctuation in time which tends more to accelerate than to retard the velocity of accommodation." The researches which I had carried out seemed to show the opposite result and in conjunction with Mr. Seashore the experiments were repeated.

Apparatus.

The apparatus was virtually that used by SEASHORE with, perhaps, some improvement. As my experiment was to determine the effect of fatigue, only one mode of accommodating was used, that from far to near.

The distance remained the same. From the far object to the near object the distance was 825^{cm} while from the near object to the eye it was 23^{cm}. The far object was a large capital E with a height of 25^{mm} and the near object was a small capital E with a height of 7^{mm} fastened on the slide of the camera shutter. The far point remained stationary while the nearer was presented or removed by a sudden movement of the shutter. A brass tube extended from the eye to the shutter. When the slide was up, the eye could see nothing except the large letter on the white back-ground. When the slide was down only the small letter could be seen. When the shutter fell, an attached copper wire made an electrical contact with a wire carrying a current through the spark coil to the battery. The other wire extended from the shutter through a closed circuit reaction-key, by means of which the current could be interrupted. The other arrangements for recording were similar to those used by BLISS.[1]

The slide was raised and the observer focused on the far object. The experimenter touched a key which was electrically connected with a sounder in the recording room. The recorder pushed down a key which closed the primary circuit. The experimenter now snapped the slide; this made a dot on the smoked paper and then closed the circuit. As soon as the observer saw the small E clearly and distinctly, he broke the circuit by the reaction-key, thus making

[1] BLISS, *Investigations in reaction-time and attention*, Stud. Yale Psych. Lab., 1892–93 I 1 (7).

another dot on the smoked paper. When the recorder saw the two sparks, he raised the key and kept out all further sparks until the fifth repetition of the experiment in quick succession. A record was thus made of the time of every fifth experiment. The wave lengths between the two dots could easily be counted and the time of accommodation obtained. This time included the subject's regular reaction-time.

The observer was asked to use his stronger eye. The other eye was kept closed by the hand or by a bandage, as he might choose.

He sat with his back to a window which admitted excellent light on the nearer E. The large E was illuminated by reflected light from a window in an adjoining room. Only those persons were used who were accustomed to being experimented upon. Only a few records were counted before all the experiments were completed; thus no suggestion of the result could enter as a prejudice. Of the six persons taken, two had according to their own previous statements very healthy eyes, two the average, and two below the average; I thus made sure that no diseased eye was used. All were faithful observers.

Five sets of records in hundredths of a second were made on Mr. Seashore and myself; the averages are shown in Table VI. In order to compare my results with those taken in 1893 by Mr. Seashore, every tenth experiment was recorded in my case.

The average duration of a set of experiments was twenty-seven minutes. Each set contained 300 accommodations or more. On account of the shortness of the drum only 70 accommodations could be recorded; this was the original reason for recording every fifth result.

In the experiments on Mr. Seashore the time is decreased for the first four records; this may be due to the fact that attention increases during that period. The curve then continues to rise till the thirty-fifth is reached. From there to the close very little increase is perceptible. This phenomenon is in perfect accord with the results previously spoken of. The experiment closed before the work became painful.

One of the persons experimented upon by Seashore in 1893 was myself. My record was the most nearly complete which he obtained. It was made May 4, 1893. Each tenth accommodation was recorded. Table VI shows the record complete. A slight increase is evident. Fatigue had not manifested itself in any painful sensations. The note made then I copy here entire, "I found no trouble in seeing

the object at any time. I held my right eye closed by its own lid muscles. When I left the chair and looked at the objects in the room, the eye could not accommodate so easily as usual, although I felt little fatigue while at the tube. The temperature of the seeing eye was higher than that of the other. At one time I felt a little pain in the upper part of the eye but my attention was called to something else and I felt the pain no more. When I quit reacting,

TABLE VI.

No. of exper.	1	6	11	16	21	26	31	36	41	46	51	56
Seashore	35	34	32	29	33	38	45	42	44	60	50	50
Moore, 1893	36		35		36		30		26		32	
Moore, 1894	30		31		32		31		31		52	
No. of exper.	61	66	71	76	81	86	91	96	101	106	111	116
Seashore	41	49	44	61	58	54	64	57	58	51	48	38
Moore, 1893	37		35		32		30		39		35	
Moore, 1894			33				46		48		42	
No. of exper.	121	126	131	136	141	146	151	156	161	166	171	176
Seashore	54	63	70	40	56	62	58	65	67	64	77	70
Moore, 1893	46		34		39		38		55		37	
Moore, 1894	48		58		42		51		41		53	
No. of exper.	181	186	191	196	201	206	211	216	221	226	231	236
Seashore	58	54	61	75	68	61	79	82	72	62	65	58
Moore, 1893	54		45		39		30		34		48	
Moore, 1894	70		65		71		79		57		98	
No. of exper.	241	246	251	256	261	266	271	276	281	286	291	296
Seashore	58	51	55	69	61	64	47	55	71	62	62	87
Moore, 1893	34		31		45		48		40		43	
Moore, 1894	64		53		61							
No. of exper.			301	311	321	331	341	351	361	371	381	391
Moore, 1893			43	42	43	45	43	50	47	54	43	46

I thought I could have gone on much longer, but I found that for twelve minutes my eye felt the fatigue, and at times a slight pain, or at least uneasiness, was perceptible in a part of the eye."

From these it is easily seen that fatigue had not appeared to any great extent. On Aug. 8, 1894, other experiments were made similar to the former ones. My physical condition was not so good as when the first was taken. Each tenth accommodation was recorded. I was unable to go beyond the 261st experiment because of severe pain in the eye. Table VI. gives this record also. The record begins with 0.30^s and closes with about 0.61^s. The time is doubled. Fatigue had thus greatly increased the time.

The three results are shown in fig. 13.

Other experiments, not included in the table, were made in which every fifth accommodation was recorded. They begin with about 0.40^s and close with about 0.85^s. These records show only about 240 accommodations in a set, as the eyes had been weakened by other work. The farther object would become indistinct, or pains would give warning to close. The experiments on myself show conclusively that fatigue does increase the time of accommodation.

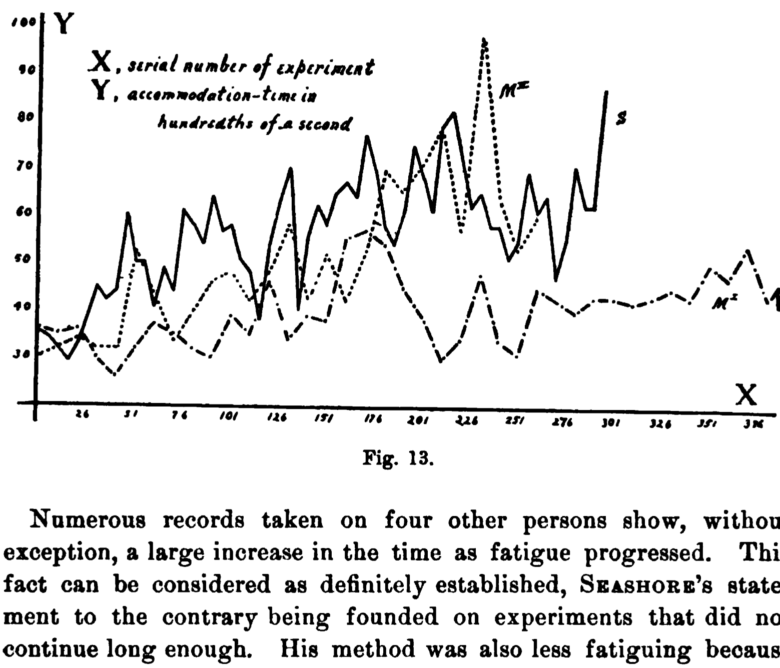

Fig. 13.

Numerous records taken on four other persons show, without exception, a large increase in the time as fatigue progressed. This fact can be considered as definitely established, Seashore's statement to the contrary being founded on experiments that did not continue long enough. His method was also less fatiguing because he allowed more time between each accommodation.

Since only every fifth accommodation was recorded, the rhythmical movement which was so prominent in the previous work does not appear so plainly in this. The records in this seem to be much more irregular than in the preceding work. Mr. Seashore, however, made this note among other introspective observations. "There seemed to be a rhythmic fluctuation of the time and effort it took to accommodate clearly." This introspective observation is corroborated by that of another observer. Could the experiments have been recorded singly it is highly probable that the rhythmic fluctuations would have been fully as prominent as in any other work. If the rhythm is due to attention there is no reason why it should not appear.

Effect of fatigue on the maximum rate of voluntary movement.

Dresslar[1] found that the time of making 300 taps varies with the mental and physical condition of the individual, and that fatigue caused a decrease in rapidity when the experiment was extended beyond 300 taps. Bliss[2], with superior methods, showed the effect of changes in the state of attention upon the rapidity of making taps, and proved that fatigue caused a decrease in the rate soon after 100 taps.

In the following experiments the methods were still further improved. The subject was seated in an isolated room which admitted neither light nor sound except when desired. There was a table of convenient height on which to rest the arm. The arm was strapped firmly to a block of wood reaching beyond the elbow. The end of the block was so cut and shaped as to fit the palm of the hand, and thus to let the hand grasp it without any inconvenience. No part was free to move except the index finger. A rod clamped to the table acted as support for a reaction-key previously described.[3] This key was fastened upside down by the clamp. The primary circuit for the spark coil passed to the binding post at the bottom of the key and to the movable slide. A spark was thus made at every upward movement of the slide. The registering was done on the drum by sparks directly on the time-curve written by a marker connected with the 100 v. d. electric fork. With the key thus connected the circuit was closed when the finger was in a partially bent or natural position. The adjustable slide was set so as to give an excursion of 5^{mm}. The record was made when the finger began an extensor movement.

The subject was to begin when warned by a click from the sounder at his side and to continue to tap as rapidly as possible until his finger could move no more, or until told to stop. The finger was never completely fatigued because sufficient records could not be gotten upon the drum. I once made 480 taps but am quite sure that I could not have made 100 taps more at the close of any record, for it was always with great difficulty that the last 50 or 75 were made.

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[1] Dresslar, *Some influences which affect the rapidity of voluntary movements*, Am. Jour. Psych., 1891 IV 514.

[2] Bliss, *Investigations in reaction-time and attention*, Stud. Yale Psych. Lab., 1892–1893 I 45–49.

[3] Scripture and Moore. *A new reaction-key and the time of voluntary movement*, Stud. Yale Psych. Lab. 1892–93 I 88.

The usual experience was as follows: Pain was first felt in the muscles of the forearm, then in the upper arm. Suddenly the blood rushed to the face and head, and the temples were filled with sharp pains. The whole right side (the work was done with the index finger of the right hand), seemed to partake of the trouble. After the experiment was completed, the arm seemed paralyzed. I could not even handle the record-sheet with safety. The arm, especially the forearm, and often the shoulder would feel painful for a half hour afterward. Restoration was not complete in less than two hours and a half.

Three records were made on myself. The individual taps of each record were placed in successive groups of 10, and the average of each group was calculated. For the final results the average was taken of the three original averages for each group. Such a condensation of results was necessary because the 3 records gave $480 \times 3 = 1440$ individual records whose relative values could not well be grasped, and also because the changes within a group of 10 were so small that the course of the fatigue function became evident to the eye only when the unit of the argument was made larger than a single tap.

The final averages in my own case expressed in thousandths of a second, ran as follows: 200 195 192 194 198 198 208 221 229 221 230 238 230 231 235 240 244 244 248 246 257 241 273 254 256 248 240 240 246 243 249 263 255 301 290 276 275 277 321 320 300 340 325 322 288 293 308 359.

In the records the fatigue is manifested by the time the seventieth tap is reached. In the final average it is clearly shown at the seventh group, which includes the 71st to 80th taps.

A professional cornet player, who had not played for a year, showed no fatigue before 150 taps had been made. A woman who played on the piano, showed no fatigue until 200 taps were made. She affirmed that she felt no fatigue, but the incipient fatigue had left its traces in the records beyond the 200th tap. There was a very slow increase, and consequently even at the end it was not very large.

In my record the rate of tapping increases for the first three groups of ten. Then there is a decrease in the rate for the next six groups. Then it increases slightly, and then decreases again. A rhythm seems to manifest itself. It is seen better in the individual records like the one shown in fig. 14 than in the general one.

A summary of the figures of the experiment made Nov. 11, 1893,

10 A. M., is given in Table VII, as illustrating the process of fatigue. In this experiment 470 taps were recorded. These 470 taps were put in groups of ten and their averages taken. By comparing these averages, the rate of tapping, and its increase or decrease, are readily seen throughout the series. N gives the number of the set of ten records.

The mean variation was found by taking the average of the individual deviations from the average of its own group of ten. This does not always increase steadily with increase in the average; but after fatigue has become very prominent the variation becomes very large.

TABLE VII.

N	Av.	Mv.	N	Av.	Mv.	N	Av.	Mv.
1	193	8	17	338	13	33	304	94
2	191	9	18	239	16	34	279	67
3	179	6	19	249	12	35	322	75
4	193	12	20	246	9	36	238	12
5	191	11	21	239	15	37	231	15
6	189	12	22	238	5	38	293	72
7	197	7	23	227	12	39	288	37
8	217	25	24	250	22	40	239	13
9	210	9	25	238	13	41	302	66
10	206	16	26	245	23	42	314	69
11	222	20	27	234	12	43	336	91
12	226	9	28	233	16	44	295	33
13	228	9	29	232	11	45	277	33
14	212	21	30	221	11	46	304	56
15	230	6	31	226	16	47	406	75
16	224	12	32	232	18			

My tap-time cannot be closely compared with those of BLISS and DRESSLAR, who simply tapped and released a telegraph key, whereas I moved a slide 5^{mm} and back. My time is that of the complete excursion, the time up, the time at rest, and the time down combined.

The results of this record on myself are presented in fig. 14. The fluctuating nature of the tapping is made evident; quite noteworthy are the extremely long taps that occur with increasing frequency toward the end. These fluctuations probably bear some relation to those observed by LOMBARD[1] when weights are repeatedly lifted.

[1] LOMBARD, *The effect of fatigue on voluntary muscular contractions*, Am. Jour. Psych., 1890 III 24.

LOMBARD, *Effet de la fatigue sur la contraction musculaire volontaire*, Archives italienne de biologie, 1890 XIII 371.

LOMBARD, *Influences which affect the power of voluntary muscular contractions*, Jour. Physiology, 1892 XIII 1.

The records thus prove that fatigue lengthens the time required for repeating the voluntary movement of tapping at a maximum rate, that the increase is not a steady but a fluctuating one and that the irregularity in the repeated movement also increases.

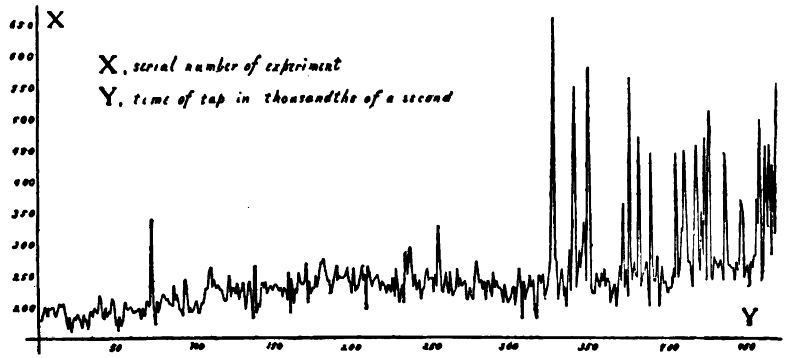

Fig. 14.

GENERAL CONCLUSIONS.

The conclusions for each kind of experiment have been given in the appropriate sections. Here I wish to call attention to the two general chararacteristics of fatigue, namely, an average change directly in the phenomenon influenced and a change in the range of variation around the average. The average change might be called the coefficient of inaccuracy and the mean variation the coefficient of irregularity ; then the law can be stated that both the coefficient of inaccuracy and the coefficient of irregularity are increased by fatigue. It would hardly be justifiable to give a more precise quantitative form to the law except by saying that the increase is most rapid at first, and then steadily slower. A final point to be noticed is the increasingly frequent recurrence of extreme results as fatigue progresses.

SOME EXPERIMENTS ON THE REACTION-TIME OF A DOG.

BY

EDWARD M. WEYER.

The psychological aspect of reaction-time may be presented in the following manner. When I am the person experimented upon, the phenomenon measured under reaction-time includes two mental processes, perception and volition ; when I measure another person's reaction-time, I infer, from analogy with my own organism, that the acts of perception and volition occurred in him also. Likewise, when the experiment is arranged so as to involve in my own case discrimination and choice, I assume that it involves similar mental processes in others.

We have every reason to believe that when the arrangements for simple and complicated reaction-times are applied to animals such as apes, dogs, etc., mental processes more or less similar to our own take place. It seems but a logical inference that the methods of experimental psychology should be applied to the study of animals ; success in systematized work in this direction ought to lead to a science of comparative psychology as accurate and well established as that of comparative anatomy.

In the present case I attempted to determine the simple reaction-time and the reaction-time with discrimination and choice in the case of a dog. The first problem was successfully solved ; the second only partially.

In determining the simple reaction-time the usual spark method was employed.[1] Pressing down the multiple-key sent a moderately strong electrical impulse to the forefoot of the dog and at the same time made a spark on the drum. The withdrawal of the foot produced another spark by means of a lever placed upon it.

The results gave : average, 89^{σ} ($^{\sigma}$= 0·001 sec.) median, 86^{σ} ; mean variation for a single experiment, 19^{σ} ; mean variation for the series of 21 experiments, 4^{σ}.

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[1] BLISS, *Investigations in reaction-time and attention*, Stud. Yale Psych. Lab., 1892–1893 I 7.

The reaction time is shorter than that for man. Average times as short as 111^{σ} have been recorded[1] for a shock in the hand. The mean variation is about that of the average untrained person.

The attempt to add the processes of discrimination and choice met with serious difficulties in the way of methods and apparatus.

The first difficulty lay in a method of getting the dog to discriminate between two objects. Complete success was found in the use of two telephones on the floor, at about a meter apart, either one of them being clicked by an electric current. Through the incitement of a piece of meat for a few times when a click occurred, the dog was led to watch carefully and to turn his head at once in the direction of the click. To keep him in a state of attention ready for either click, the person having the food stood in front so that the dog looked midway between the telephones.

The next difficulty lay in inventing a key to record the moment of movement of the dog's head, as he turned toward the click. A muzzle was placed on the head and the key screwed to the nose-piece. Two keys were made. One of them was on the principle of a small weight supported by an elastic metal rod, the rod being surrounded closely but not touched by a metal ring ; owing to the inertia of the weight, the movement of the head caused the ring to touch the rod and to close an electric circuit. The other key consisted of a small ball rolling in a metal cylinder. It made electrical connection between a small metal plate on the base of the cylinder and the inner surface of the cylinder.

For various reasons neither of these keys worked satisfactorily and up to the time the investigation was necessarily closed not enough records had been taken to establish a result. If a result had been obtained, the time for the acts of discrimination and choice could be found by subtracting the simple reaction-time from the latter one. If we admit the analogy between human life and dog life, there is now nothing in the way of finding how fast a dog discriminates and chooses except difficulties with the recording key.

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[1] v. Kries und Auerbach, *Die Zeitdauer einfachster psychischer Vorgänge*, Arch. f. Physiologie (Du Bois-Reymond), 1877 312.

SOME NEW APPARATUS.

BY

E. W. SCRIPTURE.

The constant activity of the laboratory workshop during the two years since its first productions were described[1] has resulted in over fifty new pieces of apparatus for the purposes of research and demonstration. Of these I have selected for description only a few whose fundamental ideas may be serviceable to the investigator.

Pendulum chronoscope.

The spark method previously described[2] has reduced the labor of making records of reaction-time to a point lower than obtainable by any other method recording accurately in thousandths of a second. The apparatus, however, is not portable. We set ourselves, therefore, to produce an apparatus fulfilling the following requirements : 1, accuracy to the thousandth of a second ; 2, ease in transport ; 3, readiness of setting up ; 4, quickness in reading ; 5, availability for many kinds of experiments on time. These requirements were met after eight months of labor by the first pendulum-chronoscope. Two chronoscopes were built later, but the model remained essentially the same.

The pendulum-chronoscope contains in the first place an accurately adjusted double-bob pendulum. This pendulum is held by a catch at the right hand side. In making an experiment this catch is pressed noiselessly and the pendulum starts its swing. It soon reaches a light pointer held in position by a delicate spring and carries it along. At exactly the moment it takes up the pointer it presses a delicate catch which releases the mechanism beneath the base. This mechanism is adjusted to do several things ; one of them is to drop a shutter which covers an opening in a metal plate at the back of the chronoscope. The person experimented upon is seated at the back ; owing to the curtain, he can see nothing but the metal plate

[1] SCRIPTURE, *Some new apparatus*, Stud. Yale Psych. Lab., 1892–93 I 97.

[2] BLISS, *Investigations in reaction-time and attention*, Stud. Yale Psych. Lab., 1892–93 I 7.

with the covered opening. He finds before him a rubber button like that on a telegraph key. He is to press this button as soon as he sees the shutter move. He does so, and another mechanism releases a horizontal bar running behind the scale. The pointer swings between this bar and the scale and is consequently stopped when the bar snaps against the scale. The pointer starts to move

Fig. 15.

when the shutter starts to fall and consequently any time that elapses thereafter will be indicated by the distance through which the pointer travels before being caught. The connection of the pointer with the pendulum is so light that it continues its swing (fig. 15) and is finally caught on the other side.

The scale in front is graduated by actual comparison with tuning-fork records made by the finest graphic methods. Special electrical contacts are arranged so that the units of the scale indicate always

the elapsed time between the starting of the shutter and the pressing of the button. That is to say, all lost time in the action of the mechanism is taken up in the scale. All irregularities of the instrument would also appear in the records. The vital point of the construction is, therefore, accurate adjustment; this has been so successfully attended to that the mean variable error of the instrument does not exceed 0.002^s. The scale is marked in hundredths and half hundredths; the latter space is readily divided by the eye into fifths; thus giving records in thousandths of a second.

For reaction to light, colored cards are inserted into a holder just behind the shutter, or a reflecting surface at this point receives light coming from the side and sends it through colored glass or gelatine. When several different colors are to be used, a wheel containing them is placed just behind the shutter. This wheel was left off the chronoscope shown in the figure, but considerable experience has proved that the state of mind of the person experimented upon differs greatly according as he supposes the order of presentation of colors to be left to chance or to the experimenter. To present them by chance all the compartments in the wheel are filled with two, three, or more colors as desired; the wheel is given a twirl and is stopped without any knowledge of what particular color is ready for exposure.

The reactions to light are not disturbed by noises, as the pendulum makes no noise either at release or during its swing and the shutter makes only a faint sound.

For reactions to sound without further apparatus the shutter is arranged to strike with a noise. In this case a constant quantity is subtracted from the shutter as read. For sound-reactions it is generally preferable to insert a telephone either with or without a battery in circuit with the platinum contact about to be described.

The shutter rests against a platinum point in such a way that its movement can be used to break an electric circuit; this can be used for producing lights, sounds, electric shocks, etc. A strong electromagnet is placed beneath the base in such a way that it can take the place of the button; thus the pointer can be caught by the movement of a key in the hands of a distant person. An arrangement is also provided whereby the pendulum itself is released electrically. Still further mechanisms are added for various purposes.

I may add that the chronoscope has fulfilled all expectations; in particular, the fact of its constant readiness for use without a moment's delay has been a great inducement for employing it in research and demonstration.

Standard drum.

For records where great constancy of revolution is desired we use the standard drum, fig. 16. This consists of a very heavy wheel revolving on a vertical axis. The sliding carriage to the right is arranged to carry a fork or a marker. On the top of the drum there is an automatic break. A rigid arm projects from the axle; as it passes a certain point, it trips a lever and breaks an electric circuit.

In using the automatic break the drum is turned slowly till the arm just touches the lever. The marker is then run vertically downward so that its trace, the zero line, shows the true position of its point when the circuit is broken.

Fig. 16.

When the drum is used for reaction-times, the stimulus, e. g. light, sound, etc., is produced by the break either in direct circuit or shunt as the case may require. The drum is run at any desired speed, and the marker or fork draws the time line. The distance between the zero line and the reaction spark gives the elapsed interval.

The automatic break has a special, very important use in finding the latent times of markers, spark-coils, etc. For markers this must be done separately for make and break on every occasion. For spark coils used in the way described it is necessary to do this only

once. One reason is that when the multiple key[1] is used, the latent time at each end of the record will unquestionably be the same, and so disappears. Another reason appears in the results of the following experiments. The automatic break was inserted in the primary circuit of the spark coil, (1 inch Ritchie with condenser around the break) with 4 amperes of current; the secondary circuit was arranged to make a spark off the point of the time marker, vibrating 100 times per second.

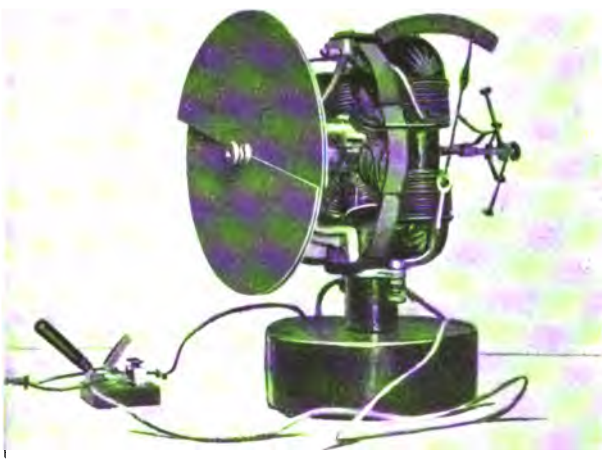

Fig. 17.

The exact position of the automatic break was marked on the paper; this could be determined within an error of less than 0.1^{mm}. The drum was then revolved at high speed and a record was taken with a spark produced by the break. The speed of the drum was so great that an interval of 0.01^{s} occupied 28^{mm}; the great regularity of the drum enabled us to treat the interval as constant throughout. The lag of the spark was measured in tenths of a millimeter. The quotient of the lag divided by the length of the wave gave the time

[1] SCRIPTURE, *Some new apparatus*, Stud. Yale Psych Lab., 1892–93 I 97.

of lag. The result of six measurements gave : mean time of lag, 0.00014^s ; mean error for the series, 0.000015^s ; mean error for a single experiment, 0.00009^s ; largest error occuring in our determination, 0.00032^s.

The mean time of lag is so small that it is quite negligible except where accuracy to the ten thousandth of a second is required, e. g. in graduating a chronoscope. The mean error completely disposes of an objection seriously urged against the spark-method, namely, that the spark jumps irregularly through the paper ; with the paper used in this laboratory (glazed paper for covering pasteboard boxes) the extreme error was only one-third of a thousandth of a second.

Electric color wheel with speed indicator.

A small series-wound motor is mounted on a very heavy iron base. A suitable arbor to hold the color discs is placed on the front of the axle. A prolongation of the axle to the rear carries two arms like those of an engine governor, which diverge from the axle according to the speed of the motor. By an attachment to a pointer this divergence can be read off on a scale. By an electric contact in combination with the spark method, this scale is graduated in revolutions per second. The speed of the motor depends on the amount of current sent through it and can be regulated at will. The apparatus is shown in fig. 17.

It also affords the means for investigating the flickering-point under various conditions.[1]

Color sight tester.

The usual tests for color-blindness fail to detect those who are color-weak, although these persons are really color-blind for objects at a distance. For examinations of railroad employes and sailors I have invented a convenient instrument which not only detects the color-blind with rapidity and accuracy but also detects the color-weak.

The use of different intensities of light for the purpose of a quantitative determination of color-blindness was, I believe, first made by Donders. In the form of a very cumbersome lantern by Eldridge Green it is now, I understand, officially adopted by the English Board of Trade.

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[1] Marbe, *Zur Lehre von den Gesichtsempfindungen, welche aus successiven Reizen resultiren*, Phil. Stud., 1893 IX 384.

In general appearance the color sight tester resembles an ophthalmoscope. On the side toward the person tested, fig. 18, there are three circles of glass one-quarter inch in diameter, numbered 1, 2 and 3 respectively. The opposite side of the tester, fig. 19, consists of a movable disk carrying twelve glasses of different colors. As this disk is turned by the finger of the operator the various colors appear behind the three windows. At each movement of the disk the subject

Fig. 18.

Fig. 19.

calls off the colors seen at the windows. The windows, 1, 2, and 3 are, however, fitted with gray glasses. No. 1 carries a very dark smoked glass; all colors seen through it will be dark. No. 2 carries a piece of ground glass, showing all colors in full brightness. No. 3 carries a light smoked glass. There are thus thirty-six possible combinations of the colors. The twelve glasses are, however, mainly reds, greens and grays.

A suitable arrangement of the colors gives direct simultaneous comparisons of reds, greens, and grays of different shades. The well-known confusion by color-blind persons of dark greens with reds, greens with gray, etc., are exactly imitated, and the instrument gives a decisive test for color-blindness. Its peculiar advantage, however, lies in the fact that it presents reds, greens, and grays simultaneously in a large number of different shades of intensity. The light of a green lantern, as it appears to a color-weak person at different distances, is simulated by the red behind the different grays; at the same time a white light is also changed. The color-weak person to whom weak green is the same as gray (white at a distance) is utterly confused and thinks that the weakened green is gray (white) and the dark gray is green.

The actual test is performed in the following manner: The tester is held toward a window, but not in the bright sunlight, at about 2½ feet from the person tested. The operator begins with any chance position of the glasses, and asks the person tested to tell the colors seen through the three glasses, Nos. 1, 2, and 3. He answers, for example, "No. 1 is dark red; No. 2 is gray; No. 3 is green." The operator records from the back of the tester the letters indicating what glasses were actually used. Suppose he finds that A, D, and G were opposite the glasses Nos. 1, 2, and 3 he records: A 1, dark red; D 2, gray; G 3, green. The disk is then turned to some other position, the colors are again named, and the operator records the names used. For example, the result might be: "No. 1 is dark green; No. 2 is white; No. 3 is red." and the record would read: G 1, dark green; J 2, white; A 3, red. Still another record might give; J 1, dark gray; A 2, red; D 3, medium gray. Similar records are made for all combinations. Of course, the person tested knows nothing concerning the records made. The blank thus filled out is forwarded to the chief inspector for railway or marine service. A comparison with a table containing the true colors for each position determines whether the test has been passed or not.

The records can be taken by any one, and, on the supposition that the record has been honestly obtained and that the instrument has not been tampered with after leaving the central office, the comparison is likewise mechanical. There is none of the skillful manipulation required in the wool test and none of the uncertainty attaching to its results. The only instruction given to the subject is, "Name the colors"; the results render the decision with mechanical certainty.

The three records just cited were all obtained from the red glass A, the gray glass D, the green glass G, and the white glass J, in combination with the dark gray No. 1, the clear No. 2, and the medium gray No. 3. Those familiar with color blindness will notice that these combinations place side by side the colors most confused. More important still is the fact that red, green, and white are made to undergo changes that detect the color-weak. Another set of colors includes red, white, green, and blue-green, subject to all the combinations. For green I use the ordinary green glass common on most railways; the blue-green is known to dealers as "signal green," and is frequent on the water. The third set of colors is an additional test. It includes orange-brown, green, blue, and violet. These colors are confused in many cases of color-weakness and color-blindness.

Thought and action apparatus.

On a horizontal rod 1 meter long, fig. 20, there are placed three metal blocks, A, B, C, adjustable at any distance apart. The block A carries a signal and is arranged so that a movement of the signal

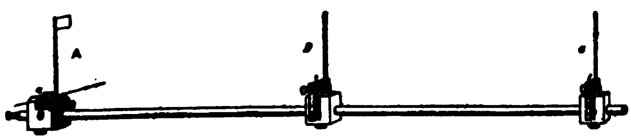

Fig. 20.

breaks an electric circuit at *a*. The blocks B and C carry light bamboo rods held in small revolving clamps. On touching one of these rods it falls and in doing so it makes electric contact for an instant at *e* or *i*. By connection with the spark coil records are obtained on the drum for each of the three movements.

Let it be required to determine the length of reaction-time in relation to the intended extent and velocity of movement. The finger is placed lightly against C and upon a signal from A it is to be moved past B. The time between the sparks from A and C is the reaction-time; the time between those for C and B is the time of movement. By varying the distance between B and C and the time of movement the problem can be answered in all its forms.

By omitting B the instrument serves as signal and reaction-key. By reacting to a movement of A in one direction and not in the other it is used for discrimination and choice. By using B and C alone it is an apparatus for repeating and extending the investigations[1] on the time and extent of movement.

[1] Fullerton and Cattell, On the perception of small differences, 103, Philadelphia 1892.

Pistol key.

This serves for investigating the reaction-time in starting to run. The blast from the pistol barrel *a*, fig. 21, moves the fan *f* so that contact is broken at *d* for an instant, the lever being drawn back by a spring. Only a few experiments have been made with the apparatus. These, however, have brought to notice the following facts: 1. the reaction-time is about one-third shorter for short-distance runners, who are trained to start quickly, than for long-distance runners; 2. the reaction-time for movements of the whole body is longer than for movements of a single member.

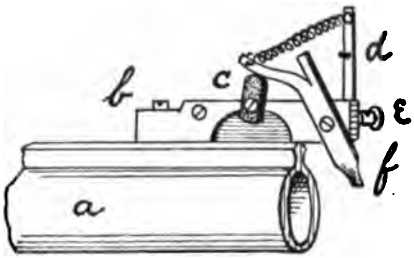

Fig. 21.

Voice key.

The voice key, fig. 22, is intended for experiments on the time of associating ideas. It comprises a cylinder of hard rubber holding a metal plate at the end furthest from the mouth. A fine screw rests lightly against this plate. The current is sent through the point of contact between screw and plate; it is interrupted by the vibration of the plate in response to the voice. With the spark method this produces a chain of dots on the drum while the word is being spoken.

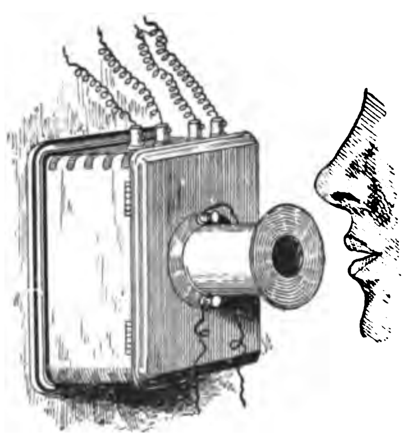
Fig. 22.

With two voice-keys and a telephone connection the person experimented upon can be in a distant room, a key being placed over the opening in each transmitter.

Touch key.

A long flexible spring, fig. 23, is mounted in a wooden handle and bears against a short rigid arm, also attached to the handle, so as to keep closed an electrical circuit. The circuit is led into the handle

by a pair of twined flexible cords. When the end of the spring touches the subject, it is pressed away from the short arm and breaks

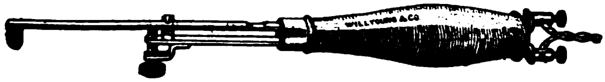

Fig. 23.

the contact. Striking against a piece of metal above, it makes contact again immediately. In this way a spark is made and the circuit is closed ready for the reaction. For touch alone the spring carries a small rubber knob at its end; for temperature this is replaced by a heated or cooled metal ball.

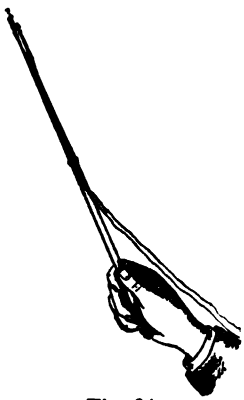

Fig. 24.

Electric baton.

This is designed for the study of the sense of musical rhythm. A rod, fig. 24, similar to an orchestra leader's baton, is provided with a spherical ball of metal mounted upon a flexible wire. Closely surrounding but not touching this wire is a metallic ring. The tip and ring are respectively joined to the ends of a pair of flexible conductors, leading off to a recording circuit. Every change of direction of the baton makes contact for an instant and thus a spark record is made upon the drum.

Fig. 25.

Foot key.

The foot-key, fig. 25,[1] is designed for the study of the sense of rhythm as involved in walking. A yoke piece is arranged to clamp to the heel, provision being made for heels of varying widths. Upon this yoke is mounted a small contact key, so that the circuit is broken whenever the foot strikes the ground and made again as the foot is raised. Flexible conductors place the shoe in the recording circuit.

[1] Figures 22, 24 and 25 are from SCRIPTURE, Thinking, Feeling, Doing; Meadville, 1895.

Volt reducers.

When the city current is at hand in the laboratory, it is very desirable to use it for forks, spark coils, telegraph keys, etc. This cannot be conveniently done on account of the high voltage of the current which arcs across the keys, burns out the platinum contacts, etc. With a battery current HELMHOLTZ[1] used a zigzag of resistance wire connected in parallel with the cup of the tuning fork in which he wished to suppress the spark. This idea has been developed by our laboratory mechanician, J. J. Hogan, into an arrangement by which the dynamo current of high voltage can be manipulated just as a number of galvanic cells. With the cells the current is altered in potential and intensity by the use of more or less of them and by their arrangement in parallel, in series or in combination. With the volt reducer the amount of current drawn from the dynamo is regulated by incandescent lamps. This current passes to a pair of binding-posts by way of the apparatus in which the current is desired, and by way of one or more coils of wire. The particular arrangement of the coils depends on the insertion of various plugs. In this way the current drawn from the binding-posts can be made of any potential from 1 volt to 10 volts and of any intensity within the limits of the particular volt-reducer. The current acts under varying circumstances exactly like that of a battery of the same potential and internal resistance.

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[1] HELMHOLTZ, Die Lehre von d. Tonempfindungen, 632, 3 ed., Braunschweig, 1877.

CORRECTIONS.

On page 23 of Vol. II the expression p. ? should be changed to p. 14.

On page 29 the second and third sentences should read: In the use of the average, each individual quantity influences the result in direct proportion to its numerical deviation from the average. The value $x_i = a$ has upon the average A an influence $\frac{a-A}{b-A}$ times as much as the value $x_k = b$.

In GILBERT's article in Vol. II. it will be noticed that in some cases the curve giving the median for boys and girls together does not lie between the curve for boys and the curve for girls, but does lie slightly outside. This is theoretically impossible. The error arose in the following way. The results were given, as usual, in tenths of the unit employed in measuring. According to the formula, p. 21 of Vol. II, the tenths obtained from a set of measurements are to be added algebraically to the whole number given as the median. In some cases the result for tenths was negative and, as Dr. Gilbert wishes it stated, these negative tenths were sometimes added instead of subtracted. The error does not affect the whole units in which the measurements were made, but it does affect the tenths which were obtained in the calculation. Therefore whenever the curve for boys and girls together does not lie between the separate curves, it should be made to do so.

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REACTION-TIME IN ABNORMAL CONDITIONS OF THE NERVOUS SYSTEM.

BY

ALFRED G. NADLER, M.D.

The object of the present research was to investigate the possible alterations in reaction-time and thought-time in diseased conditions of the nervous system. The experiments were performed upon individuals exhibiting symptoms diagnostic of one of four types of diseases. The subjects were patients applying at the University Clinic. The four types selected were neuritis, hysteria, locomotor ataxia and allied conditions, and alcoholism.

The experiments were performed on a Scripture pendulum-chronoscope, which consists of a pendulum, a pointer, a scale, a signal and a reaction-apparatus.[1] The pendulum is held at one side by a catch; when set free, it travels across the scale. As it passes the zero point it sets in motion and carries with it the pointer; the scale is divided into thousandths of a second, the pendulum and pointer being so arranged that one second elapses while the pointer travels from 0 to 1000. As it passes the zero point the pendulum springs a catch which gives the signal to the subject. This individual, all prepared, with a finger on the reaction button, presses the button in response to the signal. This locks the pointer against the scale at whatsoever point it happens to be. The experimenter has then only to read off the mark on the scale at which the pointer is set.

For simple reaction-time, the opening of a shutter on the signal board was the signal; for complex reaction-time red or white cards were indiscriminately inserted behind the shutter and the subject reacted only when the red card was seen.

The simple reaction consisted in this case psychologically of perception and volition, physiologically of the passage of a nervous impulse from the eye to the visual center in the brain, then to the arm center and downward to the muscles of the arm and hand. The complex reaction-time adds to this the two mental processes: discrimination between the

[1] SCRIPTURE, *Some new apparatus*, Stud. Yale Psych. Lab., 1895 III 98. The mean error of this instrument is two thousandths of a second; the mean variations of the records are therefore psychological quantities; see SCRIPTURE, New Psychology, 142, London, 1897.

colors and choice between movement and rest; the physiological side of these processes is unknown.

In classifying and arranging the results, the "median" was used[1] instead of the average as a basis of comparison or discussion. The median is the middle value in a set of numbers; for example, if there are 10 or 20 members, it is the average of the fifth and sixth, or the tenth and eleventh; if there are 11 or 15 numbers, it is the sixth or the eighth one.

Local neuritis.

In this group are classed all those cases in which the nerves supplying the muscles of the forearm or hand were affected, causing a partial paralysis of either hand. It comprises neuroses of a local nature due either to trauma or toxines. In all cases, one or more branches of the brachial plexus or nerves were affected. At the seat of a local lesion of a peripheral nerve or a nerve whose branches supply the periphery, the nerve is usually inflamed, that is, swollen, infiltrated and reddened. The sheath alone may be diseased or the inflammation may affect also the internal portion, under which circumstances the infiltration is more extensive and surrounds the nerve bundles. The nerve fibres themselves may not be involved, but there is an increase of the nuclei in the sheath of Schwann. The myelin is fragmented, the nuclei of the internodal cells are swollen, and the axis cylinders present varicosities or undergo granular degeneration. The nerve fibres may be entirely destroyed and replaced by a fibrous connective tissue in which fat is deposited. In neuritis due to lead poisoning and in the more serious cases due to trauma, the changes met with in the nerves are somewhat different. This is termed parenchymatous neuritis and the changes resemble closely that described as secondary or Wallerian degeneration, which follows when a nerve is cut off from its center. There is segmentation in the myelin and breaking up of the axis cylinder, with proliferation of the nuclei of the sheath of Schwann and neurilemma. The changes may be limited to the medullary sheath, constituting what Gombault has termed peri-axial neuritis. These neuritic changes are found in segments, the affected portions being separated by healthy parts; this is the so-called "segmental neuritis." In the musculo-spiral nerve, which is especially affected in lead poisoning, the parenchymatous and peri-axial neuritis are found together, the former generally being in the small branches going to

[1] Scripture, *On mean values for direct measurements*, Stud. Yale Psych. Lab., 1895 III 1.

the muscles, the latter in the main nerve trunk and larger branches. The symptoms in the following cases were pain along the arm and hand over the course of the nerve affected, several points of tenderness on the periphery, an inability to freely move the arm or forearm or hand or one or more fingers. Sometimes the flexor muscles were affected, more often the extensor. Some tactile sensation also was lost.

TABLE I.

Subject.	Disease.	S	d_S	C	d_C
W. B.	Neuritis due to injury	215	78	353	146
M. Y.	Neuritis	390	36	504	66
J. M.	"	379	16	557	34
J. W.	"	379	53	498	48
H. G.	"	314	91	442	163
L. M.	Neuritis, alcoholic	192	39	428	48
M. M.	Neuritis	407	35	525	39
J. W.	Neuritis, wrist drop	166	22	299	38
J. W.	After treatment	143	24	277	33
A. L.	Neuritis	294	61	478	87
A. L.	After partial treatment	185	14	487	47
J. S.	Neuritis, diseased arm	403	12	640	10
J. S.	Sound arm	256	20	423	12
C. W.	Neuritis, diseased arm	393	17	504	14
C. W.	Sound arm	243	14	392	5

Unit, thousandth of a second.
Number of records on each subject, 40.
S, simple reaction-time.
C, complex reaction-time.
d_S, d_C, mean variations for the individual subjects.

As would naturally be expected from such diseased conditions, the reaction-time is materially lengthened. The increase is undoubtedly due to the local lesion of the nerve. This is proven by the varying increases over the normal in the different subjects according to the extent or cause of the injury or the severity of the symptoms. It is also proven by the fact that the complex time is only so much longer than the simple reaction-time as exists ordinarily in normal conditions.

The mean variation, which is the index of regularity in the action of the patient's mind, is not greater than that found in healthy persons under the same conditions.

It was found that in patients with but one arm affected, the reaction-time was longer in the diseased arm than in the unaffected limb; in patients experimented upon during or after treatment the results improved with improved conditions.

LOCOMOTOR ATAXIA AND MULTIPLE NEURITIS.

The diseases of this group are affections of the spinal cord due to degeneration or sclerosis of one or another tract or column.

Although the results are alike in many respects, there are sufficient differences to bring out the distinction between the two diseases.

Locomotor ataxia is an affection of the nervous system characterized clinically by incoördination with sensory and trophic disturbances and involvement of the special senses, particularly the eyes. Pathologically there is sclerosis of the posterior columns of the cord, foci of degeneration in the basal ganglia, and sometimes chronic degenerative changes in the cortex cerebri. The peripheral nerves also undergo degeneration.

MARIE asserts that the primary change is a nutritional defect of the ganglion cells of the posterior root. In this disease, there is not loss of motor power, but incoördination. The motor, efferent fibres of the peripheral nerves are intact. In ordinary peripheral neuritis, both motor and sensory fibres are diseased. The ganglion cells of the posterior spinal ganglia are destroyed in tabes, but their axis cylinder prolongations in the cord undergo degeneration and atrophy; consequently a sclerosis occurs in the three ascending tracts, namely, LISSAUER'S tract, the postero-external column and GOLL'S column.

In multiple neuritis the lesions and pathological conditions are practically the same as in localized neuritis, with, of course, extension to larger areas and the involvement of more nerves and portions of the cerebral and spinal ganglia.

The symptoms of locomotor ataxia are manifested mainly by a lack of coördination. The lower extremities are principally affected, causing the characteristic ataxic gait. In advanced cases the arms and hands are involved, producing numbness or tingling in the fingers.

TABLE II.

Subject.	Disease.	S	d_S	C	d_C
C. M.	Locomotor ataxia	393	9	869	24
F. H.	" "	374	9	728	39
F. N.	" "	392	6	793	15
G. R.	" "	385	6	767	12
J. K.	Multiple neuritis	421	14	864	16
G. B.	" "	412	19	794	42
S. P.	" "	414	72	756	132

Unit, thousandth of a second.
Number of records on each subject, 40.
S, simple reaction-time.
C, complex reaction-time.
d_S, d_C, mean variations for the individual subjects.

In multiple neuritis, since the motor nerves are peculiarly affected, the symptoms are mainly those of paraplegia. The extensor muscles are affected more than the flexors, causing thereby wrist-drop and foot-drop and the peculiar steppage gait.

The simple reaction-times in these cases were markedly long, longer in those affected with multiple neuritis than in the tabetic patients. The thought-times were distinctively long also, but more so in the group of locomotor ataxia patients. The mean variations were comparatively small in all the series except in one subject.

Do these results agree with the symptoms and pathological conditions? And are they such as one might reasonably expect? I think we can answer in the affirmative.

In the first place let us compare the simple reaction-times. The results are longer in the multiple neuritis cases in this set of experiments. This is surely in accordance with expectation. The observer has simply to react and, the motor nerves being affected, the outward current travels more slowly; the result is a longer reaction-time. For tabetic patients the disturbance in coördination would also cause some lengthening beyond the normal time.

In regard to the complex reaction-time this difference between the two classes would appear scarcely sufficient to affect the complicated process.

The astonishing regularity of the simple reactions in locomotor ataxia, as shown by the smallness of the mean variations, remains an inexplicable fact.

ALCOHOLISM.

The patients which I have classified under the title alcoholism, for want of a better name, were men on the verge of delirium tremens.

They were men who had been on a "bout" for a varying number of days and appeared for treatment when on the border lines of consciousness. The condition is such as is seen by every one almost any day on the streets of a large city. It is a stage beyond drunkenness.

The patients are men who are immuned to alcohol and have drunk enormous quantities during their lives. They no longer become intoxicated according to the popular idea of that term. Their tissues and organs are probably saturated with the toxine. They are accustomed to drink gin and whiskey. Their condition would be, perhaps, best described as that due to systematic intoxication.

When they come for treatment their minds are clear and active; they are acutely anxious. Being aware of the condition in which they are, they are fearful lest it become worse. They cannot sleep and are ut-

terly worn out; tremors shake their frames. The walk is shaky and weak, but not staggering. Sleep is their necessity and without medical interference sleep will not come. Unless their need is fulfilled the condition becomes rapidly worse and the patient is soon in the throes of delirium tremens.

The pathological condition in this affection is not accurately known, but, reasoning by analogy, one cannot be far wrong in the following description: There is in the brain a shrinking of the substance with narrow, flattened and shrunken convolutions, and serous effusion in the ventricles and subarachnoid space. Some of the vessels have degenerated and blood has oozed into the brain substance. The nerve cells and fibres are wasted throughout their course. In the cord there is increased vascularity, especially in the posterior columns. The changes in the nerve fibres are those of sclerosis or fatty degeneration.

TABLE III.

Subject.	Disease.	S	d_S	C	d_C
J. M.	Alcoholism	156	15	479	28
P. M.	"	174	30	434	60
J. L.	"	164	6	464	20
F. W.	"	161	6	469	11
I. M.	"	163	9	528	18
J. B.	"	161	8	506	12
A. P.	"	168	9	453	20
P. M.	"	155	6	443	27
P. M.	"	169	9	398	7
F. S.	"	160	11	416	23
G. W.	"	195	16	507	32
J. W.	"	184	13	394	15
T. L.	"	173	13	397	8
L. M.	"	178	19	424	29
T. H.	"	163	20	415	72
T. H.	After treatment	131	5	394	22
J. H.	Alcoholism	157	8	499	10
J. H.	Partially cured	156	6	397	9
M. M.	Alcoholism	164	18	421	44
M. M.	Cured of attack	160	13	397	11

Unit, thousandth of a second.
Number of records on each subject, 40.
S, simple reaction-time.
C, complex reaction-time.
d_S, d_C, mean variations for the individual subjects.

In this group, strange to relate, the simple reaction-times are considerably shorter than in any series of experiments performed on healthy persons at the Yale laboratory. The complex times, however, are longer, that

is to say, the differences between the simple reaction-times and the complex times are larger than for the normal person. The mean variations are comparatively small in the first series, and in the latter they appear to depend upon the personal attributes of the observer.

In the experiments made upon the same individuals after treatment, the results showed a decrease in the reaction-times throughout, making the simple reaction-time less than in the normal and the complex time about normal. These results appear to show that the effect of the alcoholic toxine upon the individual is to heighten the power to perform simple regular movements, but that where a judgment is needed, the individual is at a disadvantage.

HYSTERIA.

To better understand the peculiar results of the experiments on hysterical subjects it may be well to begin with a brief characterization of the disease.

"Hysteria is a functional disorder of the nervous system, associated with excitability and a want of will power. It is manifested by uncontrollable nervous paroxysm or crises, and intermediate states of perverted nerve function. * * * The symptoms vary from mere exhibitions of excitability provoked by slight causes to prolonged and frequent convulsive attacks. Paroxysms of uncontrollable laughter or crying, without apparent reason, explosions of anger or terror upon the slightest provocation, headache, sleeplessness, attacks of trembling, flushing, chilliness, choking sensations, and, above all, unreasonable actions or complaints designed to impress the spectator with the importance or wonderful character of the ailments. All sorts of vagaries resulting from a perverted imagination assist in making up a truly kaleidoscopic clinical exhibition.[1] The symptoms vary from day to day. Upon being questioned regarding their trouble, the patients reply that "they are so very nervous." That statement covers the ground. In the majority of cases, the patient has exaggerated a slight trouble until it has assumed tremendous proportions in her eyes, and once having fallen into the habit she is with difficulty taught its error.

In this group, the reaction-times were very erratic, that being the most noticeable feature. The median for the simple reaction-time is almost normal, but the mean variation is extremely large. The complex time is increased greatly above the normal and here again the mean variation is greatly enlarged. The cause lies probably in the difficulty with which

[1] BYFORD, Manual of Gynecology, 137, Philadelphia, 1895.

the observers concentrated their attention. In some cases it appeared as if the patients had forgotten what they were attempting to do. Suddenly they would recollect and react, the time, of course, being greatly prolonged. This was especially true of the complex time. Or perhaps the patient would not be positive at first and would require a second thought to assure herself that the signal was read correctly.

The experimental results are given in the following table:

TABLE IV.

Subject.	Disease.	S	d_S	C	d_C
H. G.	Hysteria	308	91	399	197
E. S.	"	235	54	742	93
F. F.	"	287	77	779	123
H. A.	"	198	27	764	96
W. F.	"	186	17	599	51
J. W.	"	184	23	612	34
P. A.	"	198	31	744	68
C. A.	"	193	20	802	73
M. M.	"	187	26	749	91
P. H.	"	187	21	736	108
M. S.	"	483	185	641	167
D. A.	"	164	66	!	!

Unit, thousandth of a second.
Number of records on each subject, 40.
S, simple reaction-time.
C, complex reaction-time.
d_S, d_C, mean variations for the individual subjects.

It was found impossible to secure any records of complex time for D. A., as she could not refrain from reacting to every fall of the shutter, regardless of the color it showed.

SUMMARY.[1]

In order to compare the results for the different diseases the general average must be computed for each disease. The mean variation for each observer serves for this purpose the same function as the mean error of a set of physical measurements. Let $d_1, d_2, \cdots, d_k$ be the mean variations for the various individuals whose averages are respectively $a_1, a_2, \cdots, a_k$. Since each average was obtained from n experiments, the mean variations for each of the averages will be

$$D_1 = \frac{d_1}{\sqrt{n}}, \quad D_2 = \frac{d_2}{\sqrt{n}}, \ldots, D_k = \frac{d_k}{\sqrt{n}}.$$

[1] By the editor.

The weights of the averages are therefore

$$p_1 = \frac{1}{D_1^2},\ p_2 = \frac{1}{D_2^2}, \ldots, p_k = \frac{1}{D_k^2}.$$

The final average is

$$A = \frac{p_1 a_1 + p_2 a_2 + \cdots + p_k a_k}{p_1 + p_2 + \cdots + p_k}.$$

Since the number of experiments was the same in every case, the value n is constant and the same result for A is obtained by using

$$p_1 = \frac{1}{d_1^2},\ p_2 = \frac{1}{d_2^2}, \ldots,\ p_k = \frac{1}{d_k^2}$$

instead of the formulas previously given. The final average for each disease is calculated in this way.

If we consider the mean variation for an individual as a measure of his uncertainty of mental action, we can inquire for the average uncertainty of the group, which for k individuals will be

$$U = \frac{d_1 + d_2 + \cdots + d_k}{k}.$$

The average mental uncertainty is calculated for each disease.

Lastly, to complete the picture of the disease it is necessary to indicate how the individuals differ from each other in their averages. This is done by computing the statistical mean variation.

Let A be the average for the whole group whose individual values, or averages, are a_1, a_2, ..., a_k; then the individuals vary from the group-average by $v_1 = A - a_1$, $v_2 = A - a_2$, ..., $v_k = A - a_k$. The average variation of the individuals of the group will be

$$V = \frac{v_1 + v_2 + \cdots + v_k}{k}.$$

This figure indicates the homogeneity of the group, and thus gives a characteristic of the uniformity of the disease in this particular property.

The calculations have been performed and verified with the greatest care, CRELLE's[1] and BARLOW's[2] tables being used wherever possible.

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[1] CRELLE, Rechentafeln, Berlin, 1857.

[2] BARLOW, New Mathematical Tables, London, 1814.

In order to have a comparison with normal individuals I have added computations from the records made on 19 college students, each record consisting of ten experiments.

Condition.	S	U	V	C	U'	V'	B	k
Local neuritis	360	42	67	570	63	135	210	11
Multiple neuritis	418	5	4	848	63	54	430	3
Locomotor ataxia	387	8	7	786	23	42	399	4
Alcoholism	163	13	7	440	26	48	277	17
Hysteria	192	53	51	671	100	96	479	12
Normal	179	29	31	349	58	58	170	19

S, simple reaction-time.
C, complex reaction-time.
U, U', averages of the individual mean variations.
V, V', average departures of the individuals from the typical averages S and C.
B, difference between C and S.
k, number of individuals.

The following conclusions seem to be justified by the table:

1. Alcoholism shortens the simple reaction-time, hysteria leaves it unchanged and local neuritis, multiple neuritis and locomotor ataxia lengthen it.. (Column S.)

2. Local neuritis slightly lengthens the additional mental processes involved in complex reaction-time, alcoholism lengthens them considerably, while locomotor ataxia, multiple neuritis, and hysteria double and triple the normal time. (Column B.)

3. The individual's regularity is much greater than the normal in locomotor ataxia and alcoholism, and much less than normal in the other diseases. The irregularity is specially marked in hysteria for the higher mental processes. (Columns U, U'.)

4. Subjects with multiple neuritis, locomotor ataxia and alcoholism are much more distinctly marked off in respect to these tests than normal individuals. The close agreement of the seventeen alcoholic patients in regard to simple reaction-time is very remarkable. (Columns V, V'.)

Summing up by diseases, I believe it justifiable to say that in the two respects of simple and complex reaction-time they are characterized as follows:

1. Local neuritis: a poorly defined group with long simple reaction-time and great irregularity.

2. Multiple neuritis: a very closely defined group with very long time for both simple reactions and more complicated mental processes; also considerable irregularity in the individual.

3. Locomotor ataxia: a very closely defined group, slow in reaction

and in the higher processes, but astonishingly free from individual irregularities.

4. Alcoholism: a very closely defined group with accelerated reaction-time and not generally retarded complex time, remarkably free from irregularity.

5. Hysteria: an indefinite group, with normal simple reaction-time but greatly lengthened complex time, exhibiting extreme irregularity.

RESEARCHES ON REACTION-TIME.

BY

E. W. SCRIPTURE.

From time to time various problems present themselves in connection with the study of the time of mental processes. It has been my custom to assign some of these problems to special students. The work is done under my personal direction, and, almost without exception, I serve as the subject or the experimenter.

INFLUENCE OF A CONSTANT ELECTRIC CURRENT THROUGH THE HEAD.

(JOHN L. BURNHAM.)

The city supply (110 volts, direct) was used as the source of current. One wire was led to the binding post of a graphite resistance which regulated the amount of current. This graphite regulator comprised a plate of ground glass sliding in a grooved frame. Lines of different thickness were drawn with a lead pencil on the glass. By moving the glass any one of these lines could be placed under the springs connected with the two binding posts, whereby a resistance of any desired amount could be introduced into the circuit.

The current was made to pass through an EDELMANN milliampère-meter indicating directly the quantity of current used. The poles were two sponge electrodes. A commutator permitted the change in direction of the current and a liquid resistance rendered it possible to gradually apply or remove the current without shock, and without the knowledge of the subject.

In all the experiments where electricity was used, the records were so divided that the experiments without electricity (with the electrodes still in place but no current on) were interposed between two sets of the records of electricity, or else the start and finish would be made without electricity while the current was used for the middle records. Thus any lingering effect of the stimulus or any mental disturbance from the electrodes was neutralized. These experiments were conducted during the month of February, 1896.

The tests for the effect of the current were: simple reaction-time and complex reaction-time. The shutter of the pendulum chronoscope ex-

posed a colored disc and the subjected reacted by pressure on the knob at the back of the instrument[1]. For complex reaction-time one of two colors was exposed, the subject being required to react to one and not to the other.

TABLE I.

O	*F*	*ma*	*S*	*d*	S_e	*d*	*n*	*C*	*d*	C_e	*d*	*n*
S. head	1	1.0	143	11	143	15	7					
" "	3	1.0	146	15	137	7	7					
" "	3	1.6						281	49	274	45	10
" "	3	4.0								239	28	8
B., "	1	1.0	194	52	153	34	10					
" "	3	1.0	143	23	135	9	7					
" "	5	0.2	143	15	129	6	10	312	56	270	60	5
" "	13	1.8	143	18	129	15	10	260	9	256	25	5
" "	15	0.3	127	19	121	10	10					
Sm., "	5	0.2	132	12	121	18	10					
T., "	2	0.3	140	8	140	9	10					
" "	12	1.3	186	23	161	21	10	320	34	235	41	5
" "	13	1.0	149	10	139	14	10	287	29	304	53	5
D. "	1	0.5	136	29	157	23	5	264	66	272	60	5
" "	5	1.2	136	11	128	11	5	290	36	249	54	5
" "	5	7.0	142	10	117	5	5	279	19	275	27	5
" "	12	0.3	144	27	150	14	5	330	54	323	36	5

Unit, thousandth of a second.

O, subject of experiment.

F, date in February, 1896.

ma, milliampères.

S, simple reaction-time without electricity.

S_e, simple reaction-time with electricity.

C, complex reaction-time without electricity.

C_e, complex reaction-time with electricity.

d, mean variation.

n, number of experiments in each set.

A glance at the above table will show an almost universal quickening of both the simple and complex times under the stimulus of the electric current. A general quantitative statement of the amount is not possible, owing to variations in the conditions of different experiments.

It will be noticed that in the table there are some negative results with the moderate currents. Again, the effect of the electric current varies greatly on different people and seems also to affect the same person to different degrees at different times. For example, subject D., who took 7 milliampères on February 5, with no hesitation, felt that he was being

[1] SCRIPTURE, *Some new apparatus*, Stud. Yale Psych. Lab., 1895 III 98.

hurt with less than half that amount a week later and was willing to perform the experiment with only 0.3 of one milliampère.

However, in viewing all the results, the few conflicting records are lost sight of and the conclusion seems to be clearly indicated that the electric current shortens both the simple reaction-time and the complex time. Beyond this, by the introspective testimony of several of the subjects, there was a decided feeling of refreshment after the application of the electric current, the person feeling better at the close than at the beginning of the experiment. In only one case, where a high current of 9 milliampères was used (no reliable records could be taken), vertigo, double vision and the peculiar metallic taste were noticed by the subject.

From these experiments we might perhaps conclude that the brain was directly stimulated and quickened in its processes. Nevertheless, in consideration of the unusual character of the case, we prefer at the present time not to go beyond the statement that for some unknown cause the reactions were quicker with the electrical current than without it.

Influence of fatigue.

The term "fatigue" is used in different senses. It may mean the condition of body and mind resulting from the presence of certain toxic products in the blood. This kind of fatigue may arise from the activity of the organism itself in mental or bodily work, or it may arise by the transfusion of blood from an already fatigued organism.[1]

Fatigue is also used to mean a direct deterioration in the functional activity of the whole organism or of some part of it. When the just perceptible difference is being repeatedly measured in succession under the most favorable circumstances, its size may change with the progress of the series of records. A change toward a decreased difference, or finer sensibility, is called "a change due to practice;" a change toward an increased difference, or lesser sensibility, is called "a change due to fatigue."

Likewise a lengthening of the tap-time or reaction-time in a steadily repeated series—all other conditions remaining the same—would be called "a change due to fatigue." It would seem better to name it directly the "fatigue in tap-time, in reaction-time, etc.," because such expressions as "due to fatigue," "due to practice," etc., convey the impression of an explanation where none is present.

"Fatigue" is also used to indicate a complexity of sensations usually but not necessarily connected with toxic or functional fatigue.

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[1] Mosso, *Ueber die Gesetze der Ermüdung*, Arch. f. Physiol. (Du Bois-Reymond), 1890, 89.

There are thus three different phenomena denoted by the term "fatigue:" (1) a chemical change in the constitution of parts of the organism; (2) a diminution in functional activity; (3) a group of sensations. These three are usually connected. Thus the connection of the amount of work done (and the consequently available energy for further work) with the change in the nerve cells has been demonstrated by HODGE.[1] The connection between the amount of work done and the sensation of fatigue is familiar to every one. The connection between the sensation of fatigue and the actual exhaustion of the organism is not always maintained; thus, neurasthenia is treated by COWLES[2] as an exhausted condition of the nervous system accompanied by anæsthesia for fatigue.

In the following investigations on reaction-time no regard is paid to the sensation of fatigue; the problems are: (1) What are the characteristics of special fatigue in reaction-time? (2) Are these characteristics observable also in cases of general fatigue?

SPECIAL FATIGUE IN REACTION-TIME.

(A. E. VON TOBEL.)

In several different measurements closely related to those of reaction-time, e. g., tap-time[3], accommodation-time[4], fatigue has been observed. This fatigue may be a fatigue in length whereby the average time becomes longer, or it may be a fatigue in regularity whereby the mean variation becomes larger. The two kinds of fatigue do not follow the same course; in the experiments of both BLISS and MOORE the average time of tapping is lengthened long before any noteworthy change appears in the mean variation.

In the usual experiments on reaction-time an interval of about 10^s rest follows each experiment and longer intervals follow groups of 10 or 20 experiments. In this way fatigue is usually avoided. If, however, the experiments are repeated in close succession there is no possibility of

[1] HODGE, *A microscopical study of the nerve cell during electrical stimulation*, Journal of Morphology, 1894 IX 449.

[2] COWLES, Neurasthenia and its Mental Symptoms, Shattuck Lecture, Boston, 1891.

[3] DRESSLAR, *Some influences which affect the rapidity of voluntary movements*, Am. Jour. Psych., 1891 IV 514.

BLISS, *Investigations in reaction-time and attention*, Stud. Yale Psych. Lab., 1893 I 45–49.

MOORE, *Studies of fatigue*, Stud. Yale Psych. Lab., 1895 III 92.

[4] MOORE, *Studies of fatigue*, Stud. Yale Psych. Lab., 1895 III 87.

rest. Series of such experiments have been made by PATRIZI.[1] The stimulus occurred at intervals of 2" and the subject was to react as quickly as possible each time. The same characteristics were found as in tapping and accommodation, namely, a lengthening of the average time and an increase in the mean variation.

It was determined to carry the problem further, and, in reference to certain observations on methods of inducing hypnotic sleep, to determine in what particular part of the process the fatigue arose. It was proposed, therefore, to investigate the fatigue in the case of repeated flashes of light, and to determine whether it is due to the muscles of accommodation and convergence, to attention or to both muscles and attention.

The flash was produced by a small GEISSLER tube, connected with a spark coil in the adjoining room. The primary circuit of the coil passed through a modified WUNDT contact-apparatus.[2] This was so arranged that a revolving arm made contact at definite points with brass blocks in such a way as to illuminate the tube at regular intervals.

The tube was placed on a table in the isolated room and the subject was seated before it. The room is so constructed that neither light nor sound can enter from the outside.[3] The room was supplied with fresh air by a blower, operated from the floor below.

A telegraph key in the isolated room was arranged in a circuit with a DEPREZ-marker which wrote on the drum of a kymograph. A subject was told to press the key in response to each flash; nothing was said about removing the pressure. The pressing of the key caused a downward movement in the point of the marker and the release of the key a movement back to the original position. We thus have two mental phenomena recorded: the first is the reaction-time from the moment of the flash to the pressure on the key; the second is the length of time during which the subject chooses to hold the key down.

The experiments were continued during a long interval. Records were taken for a number of times at the beginning and then for a number of times at the end. Mr. von Tobel, a college senior, was the subject of the experiments.

After a sufficient number of records had been taken, the drum was allowed to revolve without being moved axially; the reactions continued as before, but the records overlapped and were not regarded.

[1] PATRIZI, *La graphique psychométrique de l'attention*, Archives ital. de biologie, 1894 XXII 187.

[2] WUNDT, Physiologische Psychologie, II 424, Leipzig, 1893.

[3] BLISS, *Investigations in reaction-time and attention*, Stud. Yale Psych. Lab., 1893 I 2.

After the desired interval the drum was again moved axially and the records were separated. The reaction-times were longer and very irregular. The time of holding was enormously lengthened. As nothing had been said to the subject concerning releasing the key, it was done semi-consciously. The time of holding was sometimes so long that the subject was apparently not fully awake. In fact, various statements of the subject showed that on some occasions he had fallen into a half-dazed condition resembling the first stage of passage toward hypnotic sleep.

The further details of the apparatus were the usual ones. The drum was timed by the JACQUET chronometer. The records were read in hundredths of a second; the next decimal arising in the averages was retained.

In the first series of experiments the room was kept dark. Both eyes were open and relaxed. The appearance of the flash caused both convergence and accommodation. In the next series one eye was bandaged in order to reduce the convergence; it is an easily demonstrable fact that the closed eye only partially performs the movement necessary for convergence. In a third series one eye is bandaged as before, but a light is turned on in the room; as the subject looks constantly at the tube all the time, the condition of accommodation is a steady one. In all three cases the mental condition known as "attention" is present; in the third there is no noticeable muscular effort in the eyes; in the second there is at each flash a definite change of accommodation with imperfect convergence and in the first there are definite acts of both accommodation and convergence.

The first record was as follows: No light in the room; both eyes open; flashes once in two seconds—

	Reaction-time.	Mean Variation.	Holding-time.
At start	195σ	11σ	319σ
After 5^m	300	43	753
Fatigue	105	32	434

Here we see that after five minutes, or about 150 flashes, the time of reaction was increased about 60%, the time of holding down the key was lengthened about 130%, and the irregularity was four times as great. The subject states: "During the experiment I felt a strong sense of contraction between the eyes. I also found that it required great effort to keep the attention fixed on the tube. It seemed to float around and move upward."

The next record was taken under exactly the same conditions, but was continued for 10^m—

	Reaction-time.	Mean Variation.	Holding-time.
At start	224σ	10σ	163σ
After 10^m	304	32	837
Fatigue	80	22	674

The reaction-time increased about 35%, the holding-time over 400%, and the irregularity 300%. "The bodily effects were much the same as before, only more intense. Tears flowed freely from the eyes; the feeling of contraction between the eyes became almost painful and there was a sort of general lassitude and disinclination to move."

For the third record all conditions were the same as before, except that one eye was bandaged. In this set a series of records was taken after an interval of 15 minutes also. The results were as follows—

	Reaction-time.	Mean Variation.	Holding-time.
At start	241σ	13σ	404σ
After 5^m	328	33	758
" 10^m	297	25	859
" 15^m	337	34	593
Fatigue at 5^m	87	20	354
" " 10^m	56	12	455
" " 15^m	96	21	189

"In this series I felt much greater effects than in any of the others and seemed to approach nearer a hypnotic state. There was a general feeling of fatigue all over the body, accompanied by a slight stiffness of the joints, also a feeling of floating off in the air or of dropping off to sleep."

The last set of experiments was taken with only one eye open and with a light in the room, thus eliminating the acts of accommodation and (practically) of convergence. The results were as follows:

	Reaction-time.	Mean Variation.	Holding-time.
At start	267σ	24σ	176σ
After 5^m	278	10	184
" 10^m	296	10	652
Fatigue at 5^m	11	–14	8
" " 10^m	29	–14	476

The results apparently show that fatigue of attention alone produces —at least within the first 10^m—very little lengthening of reaction-time. The increased regularity—"practice"—continues. The ten-

dency of the subject to finally "fall asleep" over his work is shown in the greatly increased holding-time in the last case.

A comparison of all the results seems to indicate the following conclusions:

1. The fatigue in reaction-time increases with the complexity of the adjustments required for perceiving the stimulus. There is least fatigue when only an effort of attention is involved, more when the act of accommodation is added and still more when the act of convergence is also added.

2. The tendency of the subject to fall into a condition of daze, as indicated by the holding-time, depends on the fact of repetition of the stimulus (fatigue of attention?) as well as on the fatigue from the adjustments.

The application of these results to the common methods of hypnotizing requires no remark.

General fatigue and reaction-time.

(John L. Burnham.)

Experiments on simple and complex reaction-time were made with the chronoscope in the manner previously described (p. 12). The morning records were made at 8:30, just before the duties of the day began, i. e., just after breakfast and before the first recitation in college. The afternoon records were made at 5:30, after the day's work had closed with a two hours session of laboratory work. The results of several series of experiments are given in Table II.

Table II.

	Morning.						Afternoon.						Difference.	
Subject.	*S*	*d*	*n*	*C*	*d*	*n*	*S*	*d*	*n*	*C*	*d*	*n*	*S*	*C*
Smith	119	10	60	263	38	60	127	16	60	309	34	60	8	46
v. Tobel	146	16	20	281	43	20	149	10	20	281	29	20	3	0
Burnham	128	13	80	256	29	80	141	15	80	278	27	80	13	22

Unit, thousandth of a second.
S, simple reaction-time.
C, complex reaction-time.
d, mean variation.

The bearing of this table of results is evident. The table shows an average lengthening of the afternoon records over those of the morning by 19^{σ} in simple and 24^{σ} in complex time. This is a loss of $15\frac{1}{2}\%$ in one case and of 17% in the other.

It will be noticed that general fatigue has a very small influence on reaction-time as compared with special fatigue.

It may be interesting to add that at the end of these afternoon records a few records were sometimes taken under the electrical stimulus. This resulted in a shortening, but never enough to bring the time down to that of the morning. This seemed to indicate that the fatigue of the mind by a day's work is greater than can be overcome by the stimulating action of the electrical current, at least as used in these experiments.

INFLUENCE OF TENSION ON THE REACTING FINGER.

(JOHN L. BURNHAM.)

The problem was suggested by the consideration that there might be some relation between the time of reaction and the energy of reaction.

The first set of experiments took the form of a constant tension applied to the reacting finger just before and during the experiment, whereby the reaction involved the moving of a weight. For this purpose a set of pulleys was arranged with a cord running over them. At one end the cord passed through a tape around the index finger of the reacting hand and at the other a 1000^g weight was attached. When the hand was put in position for reacting, the weight swung free. Thus there was a tension of 1000^g on the finger at the start and the whole mass must be raised by the finger as it pushed the key in reacting.

The first three records were taken on February 15 and 19, 1896. The results were so unexpected in various ways that the experiments were repeated for the observer E. W. Scripture at a later date, April 29, 1897, with the results as shown in the last record of the table.

TABLE III.

	Light.					Sound.				
Subject.	R	d	R_k	d	B	R	d	R_k	d	B
J. B.	127	9	127	6	0	131	9	123	8	8
J. D.	120	28	107	6	13	108	8	101	7	7
E. W. S.	152	26	128	11	24	172	30	136	16	36
E. W. S.	127	12	110	10	17	132	9	122	8	10
Weighted mean	132	19	118	8	14	136	14	121	10	15

Unit, thousandth of a second.
R, reaction without weight.
R_k, reaction with weight.
B, shortening due to weight.
d, mean variation.
Each figure is the average for ten experiments.

The shortening of the time is evident. Another noteworthy fact is the increase in the regularity of the reactions as indicated by the decrease in the mean variations. When errors of the apparatus and method are eliminated, the mean variation is a mental quantity expressing the subject's definiteness of perception and response.[1] This definiteness makes up a large part of the vague group of phenomena which goes under the name of "attention."

The following conclusions seem justifiable:

1. Increased definiteness of the act to be performed shortens the time required to begin it. In the case of the experiments with sound the action of the weight lay mainly or entirely in forcing "attention" to the finger to be moved. This was distinctly felt by the subject. When the weight was removed, the subject noticed the increased difficulty of attending to the movement. The shortening in time was 8^σ in the case of sound.

2. Increased definiteness of the expectant image of the sensation shortens the time required for responding to it. In the experiments on sight the stimulus was just above the finger and any increased attention would include it also. It could be directly observed introspectively that the strain on the finger forced attention to the place just behind it where the signal was to appear. The decreased ease of attention when the weight was removed was noticed here also.

3. The reaction-time decreases as the mental tension increases. This follows from the preceding conclusions. It is still more strongly brought out by the following facts:

a. Reactions with the pendulum chronoscope are always quicker than with other methods. This can be seen by comparing these figures with those obtained (p. 17) by use of the graphic method and the isolated room. This fact has been repeatedly noticed on various persons. An example of the short time required is seen in the average of 179^σ for 19 students (p. 10). A similar statement is true of GILBERT's results with his reaction-board.[2] The explanation is not hard to find. When the subject is placed in a quiet room away from all excitement,

[1] This view, definitely advocated and explained in The New Psychology, London, 1897, has for several years been part of my regular teaching. It is implied in the calculations published by GILBERT in Stud. Yale Psych. Lab., 1894 II 77 etc., and by MOORE in the same, 1895 III 76. Definite explanations of the meanings and relations of what I have termed the "individual mean variation," and the "statistical mean variation" are given in the Zt. f. Psych. u. Physiol. d. Sinn., 1896 X 163; see also the summary to NADLER's article on p. 8 above.

[2] GILBERT, *Researches on the mental and physical development of school-children*, Stud. Yale Psych. Lab., 1894 II 78.

there is nothing for him to do but to sit at ease till the warning for work arrives, and he falls into a more or less comfortable or relaxed condition of mind and body which is decidedly contrasted with that experienced with the apparatus and experimenter at his very face. The tense condition of mind under such circumstances is very evident to every one who reacts at the chronoscope.

b. The shortening of the reaction-time becomes especially marked in reactions to sight. The presence of the sight-shutter just above the reacting finger is conducive to the strictest attention. The reactions to sound do not gain in a similar manner, and thus it frequently results that a subject's reaction to sight is shorter than that to sound.

Influence of the amount of effort.

(Gerry R. Holden.)

The experiments with the strain on the finger, reported in the preceding section, had been planned for the purpose of solving the problem of the influence of the amount of effort on the time required for reacting. The records proved from the start just the reverse of what was expected, and it soon became clear that the constant strain on the finger produced an increase in attention which entirely overbalanced any effect of the increased effort.

Experiments were now planned in which there was no strain on the finger before the reaction and in which the subject was placed in the isolated room utterly away from the apparatus. Both factors of the increased attention in the previous experiments were now eliminated.

The reaction-key was made from a double contact telegraph key by lengthening the rear arm of the lever. A cord was attached to the lever and weights could be hooked at the end. The back contact of the key supported the weight until the knob was pressed. Pressure on the knob broke the contact, and lifted the weight at the same moment.

The graphic method of recording by means of the multiple key was used.[1] Pressure on the multiple key in the experiment room closed a sounder circuit, broke the primary circuit of a spark coil and immediately closed it again. A spark was thus made on the time-line of the drum at the moment of sending the current through the sounder, which, with a correction for the latent time, gives the moment of the sound in the isolated room. The subject broke the same primary circuit and made a second spark on the time-line.

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[1] Bliss, *Investigations in reaction-time and attention*, Stud. Yale Psych. Lab., 1893 I 10. The latest model of this key is described below among the new apparatus.

The subject's finger rested upon the key with no exertion and no tension of the muscles. Experiments were made at intervals of 15^s. Each experiment was preceded, as usual, by a warning. An interval of about 10^m (during which the ventilating blower drove fresh air into the room) was taken for rest after each set of 25 or 30 experiments. Smaller rest intervals occurred as the weight was changed. Practice and fatigue were compensated in the usual way by the order of the experiments.

Each set of records for any one weight was preceded by experiments (not recorded) with the same weight in order to produce in the mind of the subject a definite idea of the effort to be exerted.

TABLE IV.

	100^g			200^g			500^g			1000^g			1500^g		
	R	*d*	*n*	*R*	*d*	*n*	*R*	*d*	*n*	*R*	*d*	*n*	*R*	*d*	*n*
Holden	168	17	47	171	13	19	191	22	21	173	12	34	166	15	19
Scripture	179	12	14	232	2	2	218	35	9	260	45	7	203	19	15
Seashore	165	24	6	—	—	—	—	—	—	—	—	—	187	22	8

Unit, thousandth of a second. *d*, mean variation.
R, reaction-time. *n*, number of measurements.

The conclusion to be drawn is different from the one expected. No definite relation is to be found between the amount of effort and the time of reaction, the results being irregular and contradictory. Careful observation showed the source of the irregularity. The finger being in a passive condition, it was necessary for the muscles and joints to do considerable work before the movement began. Moreover, the soft tissues at the end of the finger would yield considerably before the key would move. Not only was time consequently lost, but the complicated adjustments, especially for the heavier weights, rendered the records irregular. Thus, although the method of obtaining the definite effort had been found, the method of recording the result was not adequate for the purpose for which the work was undertaken. The figures, however, prove one very important fact, namely, that the tension of the spring of the telegraph key alters the record for the reaction-time. Some definite standard tension must be adopted if results by different observers and on different occasions are to be comparable. The tension of o would appear to be the best for adoption. This is the case in the slide reaction key[1] and in a telegraph key adjusted so that the back contact rests in place with little or no tension of the spring.

[1] SCRIPTURE AND MOORE, *A new reaction-key and the time of voluntary movement*, Stud. Yale Psych. Lab., 1893 I 88.

The next step was to eliminate the effect of strain on the finger. This was done by having the subject always react with a key of no tension; the telegraph key was used in order not to introduce any further change. The finger necessarily remained passive, as the key responded to the slightest movement. The subject reacted in alternate sets with two degrees of voluntary effort. The degrees were defined as "light" and "strong;" the intention was to make the effort correspond somewhat to the extremes of o and the heaviest weight in the previous set, but no measurements were made on the actual energy of effort.

The results are given in Table V.

TABLE V.

Subject.	Light effort.			Strong effort.			
	R	*d*	*n*	*R*	*d*	*n*	*D*
Holden	158	10	26	143	8	27	15
Scripture	179	16	17	154	19	18	25
Seashore	168	15	9	137	21	6	31
Fisher	626	25	5	375	41	6	241

Unit, thousandth of a second.
R, reaction-time.
d, mean variation.
n, number of measurements.
D, decrease in *R* for strong as compared with light effort.

The figures for the colored janitor, Fisher, are interesting. Having been accustomed for several years to serve as subject in exercises and investigations, he is perfectly at home in the work and yet has no interest or concern in the experiment beyond carrying out the instructions; these facts make his record very reliable. His unusually long reaction-time has been observed for several years.

The problem has thus found a definite solution: the intensity of the effort affects the reaction-time, making it shorter for the greater intensity.

SIMPLE AND CORTICAL REACTION-TIME.

(HOWARD F. SMITH, M.D.)

The determination of the time of a motor response to a direct stimulation of the cerebal cortex and a comparison of this time with the simple reaction-time of the subject would apparently lead to certain conclusions concerning the relation between mental and cerebral processes. The attempt has been made in the following manner.

A cat was held quietly in the hands of an assistant. A double pointed platinum electrode was rested against the skin at a suitable point. A

touch key[1] was rested against the same member in such a way that a movement would break an electric circuit. The electrode was connected with the secondary coil of an inductorium; an interrupted current was sent through the primary coil. The inductorium was so connected with the chronoscope that the electrodes stimulated the skin as the index passed the zero point. The touch key was connected with the magnets that stop the index. Thus, when the pendulum was released, a moderately noticeable (but not painful) electric shock was given to the foot or the lip, and the consequent reaction by withdrawing the leg or raising the head broke the circuit of the touch key and made a record on the chronoscope.

The first experiments were made on a cat weighing nearly three kilograms. The stimulus was applied (1) to the right fore foot with the key against the elbow; (2) to the right hind foot, with the key pressed against the heel; (3) to the upper lip with the key on the top of the head. The results were (*d*, mean variation; *n*, number of experiments): right fore foot, 96^σ ($^\sigma = 0.001^s$, $d = 26^\sigma$, $n = 7$); right hind foot, 116^σ ($d = 38^\sigma$, $n = 4$); lip, 61^σ ($d = 9^\sigma$, $n = 7$).

The next experiments were made with a cat weighing four kilograms. The results were: right fore foot, 41^σ ($d = 2^\sigma$, $n = 3$); right hind foot, 62^σ ($d = 0$, $n = 4$); lip, 62^σ ($d = 8^\sigma$, $n = 5$).

With the second cat we proceeded to a determination of the cortical reaction-time by etherizing the animal in the usual way.

Before the operation was begun but after the etherization (surgical degree) had been effected, the experiments on sensory-motor reaction were again tried with the same intensity of stimulation. No response was received anywhere except from the ear, 57^σ ($d = 8^\sigma$, $n = 2$).

It is a curious fact that the muscle here involved, the *Retrahens aurem*, is one which man has practically lost the use of. When the temporal muscle was laid bare by the operation, it was found to respond, when stimulated directly, with a time of 63^σ ($d = 11^\sigma$, $n = 3$).

The cortex was then exposed by the usual surgical procedure. The motor centers were found by the electrode and the key was applied in such a manner as to record the movements. The movements produced, however, were not quite the same as those used for the sensory-motor reaction. The results were: supination of right fore leg, 184^σ ($d=7^\sigma$, $n=2$); advance of right hind leg, 161^σ ($d=26^\sigma$, $n=6$); raising of head, 33^σ ($d=17^\sigma$, $n=2$); elevation of right side of upper lip, 37^σ ($d=6^\sigma$, $n=6$); closing of right eye, 61^σ ($d=11^\sigma$, $n=3$).

We notice, first, the remarkably quick reaction of the cat, being as quick

[1] SCRIPTURE, *Some new apparatus*, Stud. Yale Psych. Lab., 1895 III 108.

as 41^σ for the right fore foot. This may be compared with that of 89^σ for a dog about twice as large as the cat[1]; it is far below the time for human beings, which rarely falls as low as 100^σ. We next notice that the second cat, though larger, was much the quicker in each kind of reaction. In both cases the hind foot was 20^σ slower than the fore foot. This is analogous to results obtained by CATTELL and DOLLEY for human beings.[2]

Etherization destroys the reactions, presumably by cutting off the sensory half. It seems, also, that it seriously affects the motor centres, as the cortical reaction for the hind leg was 100^σ longer even than the complete reaction before etherization. It would be difficult to draw any conclusion concerning the relative portion of the complete reaction-time, which is used by the cortical reaction. Even if we assume that the cortical time is one-half of the complete time (probably by far too great a proportion), we have $33^\sigma \times 2 = 66^\sigma$ for the head-movement corresponding to the lip stimulation with raising head, which yielded only 41^σ. The discrepancy is undoubtedly due to the retardation caused by the use of ether.

In conclusion we may point out the peculiar importance of such researches as these for physiological psychology. For experimental psychology as applied to man the simple reaction-time consists of a process or sensation (perception) and one of volition. For physiology the reaction consists in transmission of the irritation to the brain, various processes in the brain and transmission of an impulse to the muscles. What is the relation between the two sets of simultaneous phenomena? As an example of some of the problems that present themselves in this respect, we may mention that of the character of the "motor centers" of the cortex. Are they truly motor centers governing the muscles directly or are they, rather, sensory centers from which impulses proceed to motor centers at lower points in the brain? In terms of time are they connected with earlier or later processes in the reaction? We may hope some day to answer the question by experimental means.

Perhaps the most important bearing, however, of these experiments is to be found in the fact that, following WEYER's investigation, they show the possibility of applying some of the psychological methods to the study of animals. It is the firm belief of the editor of the Studies that a quantitative science of comparative psychology can be established by the proper development and modification of the methods of experimental psychology.

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[1] WEYER, *Some experiments on the reaction-time of a dog*, Stud. Yale Psych. Lab., 1895 III 96.

[2] CATTELL AND DOLLEY, *On reaction-times and the velocity of the nervous impulse*, Memoirs of the U. S. Nat. Acad. of Sciences, 1896 VII 404; previously summarized in the Psych. Rev., 1894 I 159.

INFLUENCE OF THE RATE OF CHANGE UPON THE PERCEPTION OF DIFFERENCES IN PRESSURE AND WEIGHT.

BY

C. E. SEASHORE, PH.D.

Among those who have experimentally advanced our psychological knowledge of the effects of very slow rates of change in the stimulation of the senses are HEINZMANN[1], FRATSCHER[2], PREYER[3] and SEDGWICK[4]. More or less systematic investigations have been made on changes between the slowest perceptible and the instantaneous ones by PREYER[5], HALL and DONALDSON[6], HALL and MOTORA[7], SCRIPTURE[8], STERN[9] and STRATTON[10].

Research on this subject has demonstrated several general facts: (1) The tendency of sensation to vary with the rate of stimulation is not primarily a peculiarity of any particular sense, because it is determined by general mental factors which enter into the perception of weak stimuli of all the senses in a similar manner. (2) The main value of knowledge of the laws here obtained does not lie in the acquaintance with the functioning of the particular sense organs that they furnish, but rather in that

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[1] HEINZMANN, *Ueber die Wirkung sehr allmäliger Aenderungen thermischer Reize auf die Empfindungsnerven*, Archiv f. d. ges. Physiol. (Pflüger), 1872 VI 222.

[2] FRATSCHER, *Ueber continuirliche und langsame Nervenreizung*, Jenaische Zeitschrift f. Naturwissenschaft, 1875 IX (n. F. II) 130.

[3] PREYER, *Die Empfindung als Function der Reizänderung*, Zt. f. Psych. u. Physiol. der Sinn., 1894 VII 241.

[4] SEDGWICK, *On the variation of reflex excitability in the frog induced by changes of temperature*, Stud. from the Biol. Lab., Johns Hopkins Univ., 1882, 385.

[5] PREYER, *Die Grenzen der Tonwahrnehmung*, Jena 1876.

[6] HALL AND DONALDSON, *Motor sensations on the skin*, Mind, 1885 X 557.

[7] HALL AND MOTORA, *Dermal sensitiveness to gradual pressure changes*, Am. Jour. Psych., 1887 I 72.

[8] SCRIPTURE, *On the method of minimum variation*, Am. Jour. Psych., 1892 IV 577; *Ueber die Aenderungsempfindlichkeit*, Zt. f. Psych. u. Physiol. d. Sinn., 1893 VI 472.

[9] STERN, *Die Wahrnehmung von Helligkeitsveränderungen*, Zt. f. Psych. u. Physiol. d. Sinn., 1894 VII 249 and 395; *Die Wahrnehmung von Bewegungen vermittelst des Auges*, same volume p. 321, and *Die Wahrnehmung von Tonveränderungen*, same, 1896 XI 1.

[10] STRATTON, *Ueber die Wahrnehmung von Drückänderungen bei verschiedenen Geschwindigkeiten*, Phil. Stud., 1896 XII 525.

they become accessory means by which we may investigate the involved central conditions of sensational, emotional and voluntary reaction. (3) There are probably three stages of this time influence for all senses. (a) The threshold for the preception of instantaneous change is generally lower than the threshold for any gradual change. (b) A gradual change may be so slow that it cannot be perceived in any period during which it may be studied, even though the compared stimulus may be raised to several times the intensity of the standard and in some cases produce fatal results. (c) The variation of sensitiveness in the region between these two extremes of change depends upon several complex central and peripheral conditions with reference to which it must be defined.

The present report is upon experiments in two different senses, pressure and muscle sense, in which gradual changes are compared with each other and with instantaneous change. The investigation was in progress in the Yale Psychological Laboratory from October, 1895, to February, 1897.

I. Effect of the rate of change upon the perception of increase in pressure.

The object of this first series of experiments is to determine the law of sensitiveness to increase in pressure when the increase is made at various representative rates of gradual change. The problem requires the following experimental conditions: (1) an initial, standard pressure over a definite area; (2) a uniform increase in this at several desired rates without any other disturbance of the original pressure; and (3) the means of varying the rate and amount of increase. After considerable preliminary work I found that these conditions were best fulfilled by a hydrostatic balance. The one used is constructed on the principle that a solid body, gradually immersed in a liquid, loses weight in proportion to the displacement of the liquid. It will be described in parts for convenient reference in the successive series of experiments.

Apparatus A.

1. The graduated tube. This consists essentially of a vertical glass tube, continued at the lower end by a U-shaped metal tube which is terminated by metal nozzles. The vertical tube has an inside diameter of 42^{mm} and is 555^{mm} long. The curved tube has an inside diameter of 25^{mm} and the radius of its curvature is 120^{mm}. There is an outlet at the lowest point of this tube through which the water may be conducted into

an escape tube by opening a pinch cock. The free end of the U-tube is adjusted for the insertion of nozzles to regulate the stream of water which shall pass. These are interchangeable brass cylinders inserted in the lower end of a rubber tube which leads from a reservoir of water. In the upper part they have an inside diameter of 6^{mm}, but through the lower end they have a smaller bore; the five here used vary in a series according to the standard drill gauge numbers: 60, 45, 30, 15 and 1 with diameters of 1.0^{mm}, 2.1^{mm}, 3.3^{mm}, 4.6^{mm} and 5.8^{mm} respectively. Thus five different rates of flow may be obtained by using successive nozzles. The purpose of the U-tube is to break and quiet the stream. The straight tube carries a graduated scale of heights.

2. The balance. A very delicate balance is constructed of a steel rod, diameter 2^{mm}, and length 410^{mm}, with the fulcrum at the middle. It is supported on knife-edge bearings and braced by a diamond shaped framework of fine steel wire. Light hooks are inserted at the two extremities to serve as means for the attachment of parts to be balanced.

3. The float. This is a metal tube suspended from one end of the balance beam inside of the graduated tube. It causes a displacement in the liquid as it is gradually immersed. It is smooth and uniform, in this series 8.1^{mm} in diameter and 450^{mm} long and heavy enough to retain a steady vertical position in water. Its bottom ends in a tapering hard rubber point which reduces the friction and upward pressure of the stream.

4. Stimulus rod. The float is counterbalanced on the other end of the balance by a similar metal tube which carries the pressure point on its lower end. A tube is chosen because it gives rigidity to the pressure point; it may also serve as a receptacle for weights. The point is a hard rubber cylinder, of 5^{mm} diameter, whose edges are not rounded off but dulled by a light buffing.

5. The graduated scale. Readings of the pressure are made on a millimeter scale attached to the graduated tube. The zero point of the scale is at the surface of the water when the balance is in a horizontal position and the lower end of the float is immersed to a point just above the tapering end. Since the diameter of the float and the specific gravity of the water are known, the readings of the height of the column of water in millimeters are readily converted into grams of pressure as exerted by the pressure point.

6. The guide lever. The float end of the balance may be fixed rigidly at the point of equilibrium by means of a spring lever. The place to be stimulated can be brought within a definite distance of the pressure point. Releasing the lever releases the balance which transfers the standard

weight to the point pressed at a definite time and with a regulated momentum.

7. The inlet and outlet clamps. Ordinary pinch clamps are used for these purposes.

In order to test the rate of flow through a nozzle it is necessary to apply some time-measuring instrument to the apparatus. An electric key is fitted up which makes the circuit the moment the water begins to flow. It consists of one of the above pinch clamps furnished with an adjustable make-contact.

8. The fountain. The water reservoir is placed three meters above the outlet in order to secure an approximately constant flow even when there is some difference between the levels at the two extremities. The reservoir is a shallow vessel holding 160 liters of water the surface of which can without inconvenience be kept within $\pm$ 50^{mm} of a constant point. A rubber hose of 18^{mm} inside diameter conducts the water in a vertical column to the apparatus. A piece of smaller and more flexible tube is used just above the nozzle where the inlet pinch clamp is applied.

9. The hand rest. This is a special support to be used when the outer surface of the index finger is to be experimented upon. It is so constructed that the index finger and the thumb may rest upon a support and the other fingers brace themselves firmly and comfortably so as to obtain perfect stillness of the index finger without interfering with its circulation. A wooden cylinder stands on a base board and carries on its top a hard rubber plate 2^{mm} thick. The thumb and index finger rest upon this plate and the other fingers grasp the pillar below, while the forearm and side of the hand rest upon the base.

Experiments.

The rate of change was varied in successive steps while the other experimental factors were kept constant. Five rates were selected such that the slowest was as slow as could generally be perceived upon the present standard and the fastest as fast as the present apparatus and method would admit. The other rates were taken between these two extremes so that the increase in pressure per second upon a standard of 5^{g} by the respective rates was as follows: 0.18^{g}, 1.10^{g}, 2.85^{g}, 4.83^{g}, and 6.63^{g}. The standard, or initial pressure, of 5^{g} was applied to a circular area 5^{mm} in diameter on the outer side of the middle of the third phalanx of the right hand index finger. The hand support was so adjusted that the finger in position upon it came as near the stimulus point as it could without touching, or about 0.3^{mm}.

The observer and the experimenter sat on opposite sides of the table

with an opaque screen between them. The observer occupied a comfortable position with his finger on the hand support and kept his eyes closed during the trials. By the signal "one" he was warned to be ready; after "two" the initial pressure was applied and about two seconds after this had been done "three" signified the beginning of the increase in pressure. Further instructions to the observers were as follows: "The pressure may increase and it may not; as soon as you are sure that it has increased, say 'up' as promptly as possible. Make sure that you have the same degree of certainty in all trials." This standard of certainty was fixed by a few preliminary trials. If the observer thought that he had not kept the standard of certainty or had suffered any disturbance he was required to call at once for a repetition of the trial. No observer was allowed to see the experimenter's side of the apparatus until all the experiments were completed.

To estimate the distortion due to the order of sequence of the rates, they were taken in rotation in opposite orders by successive observers, and the experiments were begun at different steps in the series in a systematic manner. They were also taken in the double fatigue series, i. e., half the number of trials on each point were made in going through the series the first time and then the rest were made by repeating it in the reverse order. A brief rest was made at the middle of the experiment; the whole lasted about one hour. The results for thirteen observers who tried this experiment are contained in Table I and are represented graphically in Figure 1.

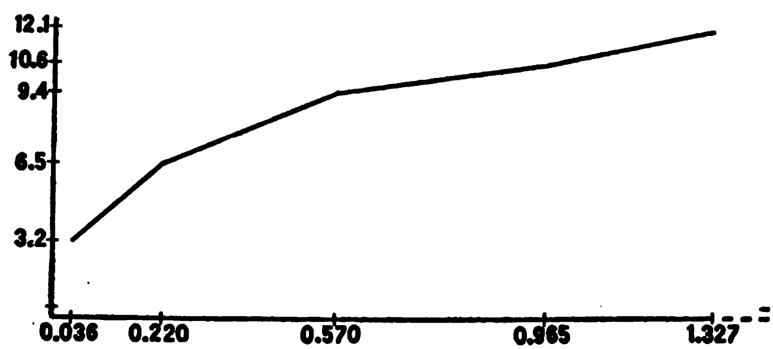

FIG. 1.

The horizontal axis in this figure is marked off into parts according to the γ data in Table I, i. e., proportional to the part of the initial stimulus by which the increase was made per second.

The lowest point marked on the curve to the left is 0.35^g which is the increment when the change is made most slowly. The same over the dotted line to the right indicates how far the curve must drop as the rate increases to the instantaneous. This result is transferred from the third series of experiments.

The law discernable in the table is this: Within the limits of the investigation the amount of the least perceptible increment rises with the increase in the rate, i. e., the faster the increase in pressure the larger is

TABLE I.

Least perceptible increase in a pressure of five grams at different rates.

	I		II		III		IV		V	
α	0.18		1.10		2.85		4.83		6.63	
β	5.55		0.91		0.35		0.21		0.15	
γ	0.04		0.22		0.57		0.97		1.33	
	$\varDelta$	d	$\varDelta$	d	$\varDelta$	d	$\varDelta$	d	$\varDelta$	d
A. B.	3.4	1.9	4.5	1.9	5.9	2.6	6.9	2.7	7.4	1.1
F. B.	5.2	1.6	4.0	1.9	8.8	2.6	8.4	1.6	7.4	1.1
S. P.	3.1	2.1	9.0	4.8	16.7	3.6	19.5	5.7	11.6	4.9
G. O.	6.3	2.4	10.6	1.5	11.4	4.6	13.4	1.5	16.9	2.6
M. A.	2.8	1.4	5.2	0.7	7.2	1.9	10.4	1.9	12.5	2.8
A. N.	2.9	0.9	4.6	1.3	9.8	2.3	13.9	3.9	15.0	3.7
E. B.	1.9	1.1	6.9	3.1	10.3	6.7	10.2	2.0	14.0	2.6
G. H.	2.6	0.9	4.9	1.6	5.4	0.9	9.8	1.3	11.4	4.4
S. K.	1.8	0.9	5.5	1.4	7.0	1.0	6.0	1.4	6.1	0.9
A. H.	1.8	1.8	6.1	1.8	6.4	2.0	8.9	0.9	9.0	2.8
P. P.	3.1	3.4	7.6	4.0	11.8	4.5	8.4	4.3	12.6	3.8
M. J.	2.7	3.7	8.2	3.0	10.4	2.1	11.4	2.1	15.5	4.2
A. S.	4.0	1.5	7.7	3.7	10.6	2.3	11.1	3.1	17.8	2.9
Average	3.2	1.8	6.5	2.4	9.4	2.8	10.6	2.4	12.1	2.9
Time	17.7^s		5.9^s		3.3^s		2.2^s		1.8^s	

The unit of measurement is the gram.

The number of measurements in each case is $n = 10$, of which the median[1] is taken.

Initials, the observers.

Roman numerals, the different rates.

$\varDelta$, the increment in grams.

d, mean variation; to find the mean variation for the series divide each of these by $\sqrt{n} = 3.2$.

α, number of grams of increase per second.

β, time to increase one gram, in seconds.

γ, part of the initial stimulus to which the increase amounts per second.

Time, the time represented by the average increment for all observers at each rate.

the size of the increment which is just perceptible. These limits include those which we experience most in normal life. But, referring to the curve, near the two ends there must be deflections of the curve in

[1] In the present research I have used the median in all the experiments with the reaction method; the average has been used with other methods. This is because, by the nature of the experiments, the variation is larger and there are more abnormal records by the former method. For the account of the median and its relation to the average see SCRIPTURE, *On mean values for direct measurements*, Stud. Yale Psych. Lab., 1894 II 1.

opposite directions if it is to be extended, i. e., if extended on the left the curve must soon reach an almost vertical direction and the extension of the other end must bend in some way so as to eventually reach the point which represents the increment in instantaneous change. The law of such deflections must be determined by future investigation.

The experiment was repeated three times upon one observer, M. J., at intervals of two weeks, each time under similar circumstances. The judgments in the successive experiments were equally unbiased, except in so far as they were influenced by the pressure sensations. The final averages for the three experiments are as follows: Rate I, Δ 2.8^s, d 0.1^s; Rate II, Δ 9.1^s, d 1.6^s; Rate III, Δ 11.3^s, d 1.6^s; Rate IV, Δ 13.1^s, d 1.2^s; Rate V, Δ 13.7^s, d 1.5^s. The results indicate that the rate influence is definite and persistent for these trials. This remarkable consistency, as well as the agreement of the thirteen observers above, can only be accounted for by assuming a definite time influence which corresponds to this variation. The general law here found has an extensive application to the whole sensory side of our mental experience and involves some disputed points. I will, therefore, give a brief critical estimate of the apparatus, the method and the conditions adopted. Much of what is here said applies, also, to the following series of experiments.

Critical estimate.

1. The apparatus. The only noticeable jarring of the apparatus came from the jarring of the building, and this disturbance was somewhat reduced by placing the experimenting table on sand bags. Furthermore the above experiments were made in the evening when it was comparatively quiet in the building. The variableness in the level of the source and the mouth of the stream caused some degree of inaccuracy. The two levels were placed so far apart vertically that the necessary variation in the level of the mouth would not materially affect the results beyond the degree of accuracy here required. The time for the water to rise through the first 100^{mm} of the graduation tube was to the time to rise through the second 100^{mm} above the zero of the same as 38 is to 39. This error is negligible because it affects all rates similarly and practically equal for proportional increments by the various rates. Since most of the measurements came within the limit of the first 200^{mm} the rates adopted were determined empirically for the average of this distance. This determination was made to a more than sufficient degree of accuracy by the graphic method of recording time.

The adjustment of the zero point of the column of water could be

made with an accuracy of $\pm \frac{1}{2}^{mm}$ which equals about $\frac{1}{28}$ of a gram by displacement. There is again a possible error of $\pm \frac{1}{2}^{mm}$, by the same unit, in the adjustment of the hand rest. Both these errors affect the standard pressure and can only affect the increment in an indirect way.

2. Method. The main reasons for using the reaction method here are: (*a*) It is time-saving, which is a vital point when a long series of records must be taken. (*b*) It does not carry with it any suggestions as to what may be desired or expected. (*c*) It is easier to interpret the results by this method than by any other. The results attained by this method need to be corroborated by other methods and that will be done, but I must here point out some of the sources of error of the method in its present application. The observer reacted with a vocal sound and I, as experimenter, made a sight reaction to that sound. This latter reaction is negligible because my eye followed the reading point as a point of regard. The sensory time in the observer's reaction should be counted to the record, i. e., the record should include the time from the beginning of the physical change to the moment it was perceived as a change, but not the motor element in the reaction. This I tried to eliminate directly in each trial by means of a subjective estimate. After some practice in sight readings of this kind I acquired some skill in estimating equivalents on the scale to the amounts lost by the observer's reaction at the various rates. I could hear the very beginning of the vocalization of the *u* in "up." But the allowance to be made had to vary with the definiteness with which this sound was uttered. I was aware of the common illusion of motion as well as of the difficulty of perceiving two simultaneous impressions in different senses. I found difficulty only in the fastest rate, but even here the possible error would be small in comparison with the whole records and the corresponding mean variations. This method of eliminating the reaction-time is not fully satisfactory, but it is superior to the previous methods that have been proposed for similar purposes.

Do we want the naïve judgment of the unpracticed but skilled observer? or the discriminative and critical judgment of the experienced observer who is familiar with the conditions and elementary processes upon which his judgment is based and gives his decision after having taken all known factors into consideration? While the latter is necessary in order to make a detailed analysis of the facts, the former is necessary for the establishment of the facts. Though it involves more than double the labor of the other method, I have made it a characteristic of this research that the facts shall be obtained as they appear without analysis in the common experience of the scientific mind. No one of my observers knew what to expect and they were expressly cautioned not

to make any guesses with consequent conscious or unconscious corrections.

The observer was directed to react when he could distinguish "change" from "no change." In this series I did not have any regular system of control experiments, i. e., trials in which no stimulus was applied. They were interspersed irregularly and by them I satisfied myself in regard to the necessary absence of illusion. The danger of illusion was emphasized in the preliminary trials. If the illusion took place then, the trial immediately following would show the trace of a reacting influence in overcautiousness and consequent missing of the standard. In such cases the preliminary practice was continued until the observer had settled upon a normal standard.

3. Conditions of the experiment. The standard pressure may seem light, but it is adapted to the place experimented upon. It was advisable to use a light stimulus which would not produce a deadening effect upon the nerves under long continued pressure. A light pressure on a small area produces a simpler and less disturbing sensation than a heavy pressure, which is liable to produce sensations of strain in parts not directly stimulated. It is also important to avoid pain.

The point upon the first finger was chosen for stimulation because it admits of being kept in a horizontal plane when the rest of the body is in a comfortable position. The place is of a good sensitiveness and free from hairs.

The slowest rate here used was determined by preliminary experiments in which smaller floats were used. It was then found that the change would not be perceived at all, in the slowest rates, even when it amounted to four or six times the original stimulus. The pressure sensation would either be entirely lost or else it would continue indefinitely to seem as but a fraction of the standard. 6.18^g per second was found to be about the slowest rate per second by which the pressure might rise and still be perceptible every time under normal conditions. The fastest rate was taken within safe limits, and rates above that were reserved for a separate test.

II. Variation in sensitiveness to change as depending on the delay of the stimulus.

In previous experiments with sight and sound[1] I found that when a stimulus near the threshold is delayed beyond a certain time at which it

[1] SEASHORE, *Measurements of illusions and hallucinations in normal life*, Stud. Yale Psych. Lab., 1895 III 36, 50.

may be expected, its threshold is lowered, and, within certain limits, this is proportional to the delay. If this law applies to pressure, a part of the facts established in the first series of experiments will be explained by it. The application of the law of delay to this particular case was tested in the following manner:

Apparatus *A* was used just as in the foregoing series but with only one rate, namely 1.10^g increase per second, which is 0.22 per second of the standard. Counting from the end of 2^s allowed for the perception of the original pressure, the increase was not begun until after a delay for the respective sets of trials as follows: I, 0^s; II, 5^s; III, 10^s; IV, 15^s; V, 20^s; and VI, 25^s. In other respects the general methods and conditions were the same as in the first series. There was no suggestion by which the observer was led to expect the change to be felt at any definite time. He was not aware of the delay; he only knew that he would feel a gradual change and that he should begin to look for it at the given signal. Nothing was stated as to when it would begin physically. He learned, however, from the preliminary trials that it would take different time-intervals for the change to become perceptible. Hence this is different from the cases of suggestion in which the observer is led to expect the stimulus at a definite moment. Here the conditions of expectation and general preparation were similar to those in the foregoing series. It is a case of suggestion that works through the variation of time-influence in ordinary perception.

TABLE II.

Variation in sensitiveness as resulting from delay of the stimulus.

	0^s		5^s		10^s		15^s		20^s		25^s	
	Δ	d	Δ	d	Δ	d	Δ	d	Δ	d	Δ	d
A. H.	7.6	3.1	6.7	2.2	4.0	2.0	3.8	2.2	3.6	2.0	4.1	1.4
P. D.	8.3	3.4	6.0	1.6	4.7	0.9	4.0	1.6	2.6	1.2	3.4	1.4
P. P.	13.2	4.5	5.1	1.2	5.1	2.5	5.8	1.9	3.2	1.2	3.5	0.9
A. B.	5.4	1.4	3.5	1.3	2.9	0.6	2.1	0.7	2.2	0.6	1.9	0.4
D. S.	6.6	3.8	4.1	2.8	5.1	1.5	5.1	2.1	3.1	2.2	4.1	1.3
J. M.	5.8	2.1	4.3	1.1	2.9	1.2	2.7	0.4	2.7	0.9	2.9	0.5
A. S.	20.0	3.0	17.1	2.9	12.9	3.0	11.9	4.1	13.4	4.3	13.1	3.7
Ave.	9.6	3.0	6.7	1.9	5.4	1.7	5.1	1.9	4.6	1.8	4.7	1.4

The unit of measurement is the gram.

The number of measurements in each case is $n=10$, of which the median is taken.

The delay of the stimulus is indicated in seconds at the head of each column.

Δ, the threshold increment.

d, mean variation; the mean variation for the series is found by dividing by $\sqrt{n} = 3.2$.

The trials were made in the double fatigue order. Surprise and the disturbance of abrupt transitions were avoided. The control trials were used freely in the preliminary trials but not during the experiment. Table II contains a summary of ten trials on each point by each of seven observers. The figures give the medians, and the average of these is taken for the final summary. There is a remarkable uniformity in the results. The abnormal record of the last observer is accounted for by the fact that his hand was callous.

The table shows that the threshold decreases, i. e., the sensitiveness increases, as the delay is extended. This law is most noticeable in the first five or ten seconds and seems to extend only to about twenty seconds.

The results may best be interpreted by means of the comparison in Figure 2. The short curve represents the results of the first series of ex-

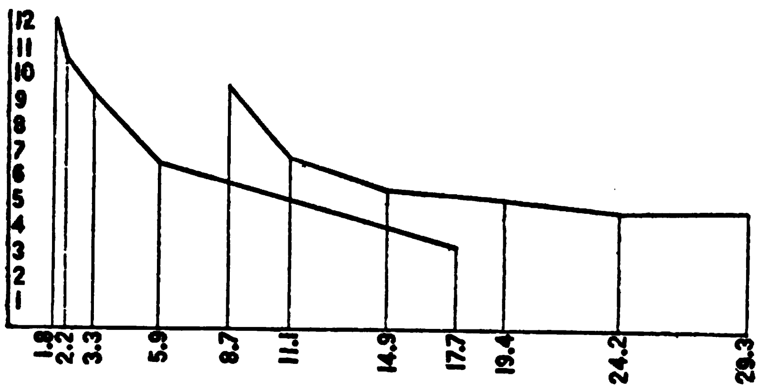

FIG. 2.

periments, showing the relation of the threshold to the time which it took to produce it. The number of seconds is laid off as the abscissa and the number of grams as the ordinate. The longer curve shows the same for the present series. Within the limits of time occupied in the first series (17.7 sec.) the threshold of difference is lowered as the time increases. Beyond that limit the delay does not seem to have any power to lessen the increment. The rate, which is actually the same in both cases, requires the largest increment in the series in which it occupies an extreme point.

A great part of this variation may be explained by the fact that the longer the time the greater the expectancy will be and the summation of suggestive elements will be on a constant increase. The smaller signs

of change come directly into the focus of attention, a greater number of them will be noticed and those noticed will be magnified. This implies that the variation of the threshold with the rate is not entirely because of the difference in impression that quick or slow rates of change make, but largely because of the different attitude of mental preparedness which is caused by different time relations. Though the observers tried to be equally attentive all the time there was a semi-conscious reinforcement of attention as time went on. When the signs of change came soon they had to compete with more rival sensational elements than if they came later. According to the conditions of the experiment there could scarcely be any surprise, but we may characterize the different states of mind by saying that in the fast rates the observer was open to conviction, while in the slower changes he more anxiously sought some imaged facts of assurance.

The fast rates are probably affected by contiguity with the slow, and likewise the reverse. The faster rates were, perhaps, at a disadvantage. It is probable that if the highest point in each curve had been established separately, without reference to any other rate, it would have been lower in both cases. Yet those would be entirely different conditions. If one rate, or time of change, is taken separately, the time for the change to be felt will be known; it will be envisaged more definitely; the attention will be sharply focused at the expected moment of change and no attention-energy will be scattered as above. Both conditions are facts of ordinary experience and it would be interesting to compare them. One form of the latter condition will be taken up in the next series of experiments.

Fatigue makes a light weight or pressure feel lighter. This is true for 5^g. How does that affect those curves or their continuation? Does it have the effect of lessening the standard and thus making the increments proportionally greater during the time extension? Or, does it work in the opposite way so that the amount of the standard, plus the increment, is constantly lessened, necessitating a longer time to make the pressure feel heavier than at first? Both are true, i. e., there is a certain limit at which the amount lost by fatigue is just equal to the increase at a certain rate. This point lies beyond the lower end of the short curve. Then the weight would feel the same *ad infinitum* if there were no fluctuations in this limit. If the change is slower, however, several possibilities of sensation-changes are open, but it is not probable that any increase in pressure will be felt before pain sets in and the experiment for pressure must be discontinued. But if the rate of increase is faster than the rate of falling off by fatigue, the standard will actually

seem smaller as time is extended, as in the present series, and the increment, which is largely detected by feelings of change, will be felt larger in proportion.

III. Threshold for instantaneous increase in pressure.

Apparatus B.

The compound pressure balance here described was constructed primarily to serve as a means by which a standard pressure over a definite area might be applied and then increased at any moment without jarring the stimulus point or causing any disturbance except absolute increase in pressure. It consists of two coördinated balances, one of which is apparatus *A* with the only exception that a tapering rubber point (*T*) is substituted for the original pressure point. The other balance consists of a light steel beam 300^{mm} long on one side of a knife-edge bearing with the balancing mass on the other side. The pressure point is a cylinder inserted at the end of the beam which is supported from a frame by a pair of electro-magnets. This part of the frame is capable of minute adjustment in height. The frame carries a millimeter scale parallel to the beam. A sliding weight on the beam carries a pointer which indicates the position of the weight on the scale; the change of weight at the pressure point (*P*) is proportional to the distance over which the weight is from the fulcrum. A light arm projects from the rear end of the cylindrical counterbalance for the purpose of affording leverage for the action of pressure from the other balance.

When the two balances are brought together, the point *T* in balance *A* is brought to bear on the leverage arm in the other balance, and can be removed by the guide lever (apparatus *A* 6) without friction or jarring. By this combination of the two balances we secure a pressure point which keeps a rigid position, a means of retaining the standard pressure constant, and an instantaneous change or a gradual change in pressure at any desired rate.

To illustrate the case of instantaneous change, suppose that the balance is set with an initial pressure of 5^g at *P*. This is supported by the electro-magnets at a definite distance ($\frac{1}{2}^{mm}$) from the surface to be pressed upon. Then if we want to prepare to increase that by, e. g., 1^g after it has been applied, the weight on the beam is moved from the fulcrum until the scale indicates that the movement is equal to 1^g at *P*. Then balance *A* is adjusted to press with a force of 1^g by *T*. This 1^g counterbalances the 1^g just added to *P* and we have again the standard weight at

P. Opening the magnet circuit always places the standard pressure with the same momentum. To obtain the 1^g increase upon the standard the point *T* is lifted vertically by a rapid movement of the guide lever (*A* 6). The 1^g is of course transferred directly to the point *P* without any movement of the beam except through the extra indentation which the 1^g causes on the skin.

I have only had time to make one of the tests for which this apparatus is intended. This test consisted in finding the least perceptible increase when the change was made instantaneously. The standard pressure and the area were the same as before; the general method was also the same. At a signal the observer got ready and about two seconds later the point *P* was applied by releasing the magnets. The increase was made about two seconds after this. The threshold was approached in both directions by steps with a constant difference of 0.2^g. The observer simply stated whether he perceived the change or not. Control trials were interspersed irregularly. The average of the complete measurements on each of seven observers is given in Table III. The figures denote the smallest increment above which all were perceived.

TABLE III.

Threshold of instantaneous increase in a pressure of 5^g.

	Δ	d
A. N.	0.26	0.08
M. A.	0.34	0.07
J. M.	0.34	0.08
G. O.	0.17	0.06
A. S.	0.64	0.15
P. D.	0.34	0.07
P. P.	0.36	0.07
	0.35	0.09

The unit of measurement is the gram.
The number of measurements in each case is $n=10$, of which the median is taken.
Δ, threshold increment.
d, mean variation; the mean variation for the series can be found by dividing this by $\sqrt{n}=3.2$.

The main value of these results lies in that they establish one end of the curve in Figure 1 as it would terminate if continued. The curve has to fall from its highest point 12.1^g to this point 0.35^g. This suggests an important problem, namely, in terms of the figure, what is the shape of the curve which must connect these two points? This will be answered for somewhat different conditions in a following series of ex-

periments. The present threshold meanwhile gives a standard in comparison with which we must interpret all the previous measurements on gradual change. Thus, the slowest gradual increase requires a threshold nine times as high, and the fastest a threshold thirty-five times as high as the threshold for instantaneous difference. In making this comparison "instantaneous" must be taken in a relative sense (as it always must) according to the above details, and it must be remembered that these two thresholds were found by different methods.

Psychologically the two judgments of gradual and instantaneous differentiation are not only made under totally different conditions of attention and expectation, but there is also an entirely different grouping of the sensations which form the basis for the discrimination.

IV. Effect of the rate of change upon the perception of increase in weight.

Apparatus C; Experiments.

An apparatus is needed by means of which gradual changes in the weight of a lifted object can be made and measured. Apparatus *C*, which was constructed for this purpose, consists of the following parts, used in conjunction with apparatus *A*:

1. The weight cell. This is a polished hard-rubber cylinder with a diameter of 21^{mm} and a height of 75^{mm}. Its own weight, 25^{g}, may be increased to different amounts by placing weights inside. A silk cord hangs from the bottom, by which weights may be attached.

2. The arm support. A board base is fixed in such a position that the arm from the elbow may rest upon it in a horizontal position. The weight cell stands on this in a position to be grasped comfortably. The cord from the cell runs vertically through a hole in the board to the stimulus end of balance *A*.

Apparatus *C* works on the same principle as apparatus *A*, the only difference being that the weight is exerted on a lifted cell instead of a pressing point. To illustrate by an example, let it be desired to obtain a measured gradual increase on a standard of 40^{g} weight in a cell. The cell itself weighs 25^{g} and we add 15^{g} by weight placed inside of the stimulus rod, to whose top the cell is attached. Since this rod is counterbalanced by the float, the 15^{g} are added to the weight of the cell when the observer lifts the cell, say 2^{mm} from the base. At that point the standard will be reached. The water in the graduated tube is then allowed to rise at some definite rate and as the float is immersed weight is transferred to the cell.

The same general method that was used in the study of pressure was here applied in the study of the so-called muscle sense, the word being used in its widest significance as including all the sensations by means of which we estimate lifted weight. The aim was to determine whether there is any law for muscle sense that corresponds to the law of rate influence that we have found for pressure, and, if so, to observe some of the relations between the two.

TABLE IV.

Least perceptible increase in a lifted weight of forty grams at different rates.

	I		II		III		
α	0.18		1.10		6.63		
β	5.55		0.91		0.15		
γ	0.005		0.028		0.166		
	Δ	d	Δ	d	Δ	d	ε
J. L.	3.3	1.1	11.5	4.0	20.3	2.1	0
V. S.	1.7	0.4	6.1	1.8	9.7	5.0	0
M. M.	5.3	1.4	11.0	2.4	18.0	4.4	0
J. P.	12.0	5.4	14.9	4.0	21.9	4.9	0
W. J.	2.4	1.0	6.6	1.5	14.8	4.9	43
A. S.	4.0	1.3	12.1	3.6	17.5	2.4	43
F. K.	3.9	0.5	12.4	2.6	17.1	5.3	0
E. J.	6.0	2.3	13.8	4.9	13.5	4.9	0
C. C.	4.0	2.4	7.3	3.4	13.7	4.2	0
Average,	4.7	1.8	10.6	3.1	16.3	4.2	
Time,	26.1^s		9.6^s		2.5^s		

The unit of measurement is the gram.

The number of measurements in each case is $n=10$, of which the median is taken.

I, II, III, rates of increase.

α, number of grams of increase per second.

β, time to increase 1^g.

γ, increase per second as a fraction of the initial stimulus.

Δ, threshold increment.

d, mean variation; to find the mean variation for the series divide by $\sqrt{n}=3.2$.

ε, percentage of the control trials in which illusions occurred.

The standard was taken as 40^g, because that weight in the given cell is favorable for a distinct feeling of weight and does not cause noticeable fatigue very soon. Only three rates were employed and these were the same as I, II and V in the first series; they gave an increase of 0.18^g, 1.10^g and 6.63^g per second, respectively.

The observer shut his eyes and grasped the cell at the middle between the thumb and the first two fingers in such a way that when he had raised

the cell 2^{mm} from the base the side of the hand and the little finger rested on the base by their full length and the cell was held upright. The position was comfortable and could easily be retained for the required time. The observer grasped the cell as lightly as possible. As soon as the correct position had been secured the signal was given which meant that except in the control trials the physical change would begin in two seconds. The reaction was made by saying "up" and the results were read as before. Nine persons made the complete experiment, which consisted of ten trials on each rate, exclusive of the control trials. The summary is contained in Table IV.

From one to five control trials were interspersed with each ten regular trials. The column giving the percentage of these trials which resulted in illusions gives an index to the reliability of the discrimination. All but two are perfectly reliable in that they have chosen the standard threshold so high that there is no danger of confusion. The high percentage of errors for the two must, however, not be interpreted to mean that such a percentage of the total number of trials may be considered as illusions, for in each case there were thirty trials in which the regular change was made and only seven in which the stimulus was withheld. All three illusions out of the five possible were in rate I for W.J., and for A.S. two out of four possible were in rate I and one out of two possible in rate II. The value of these percentages depends upon the relation between the number of control trials to the number of regular trials as well as upon the degree to which a trial with no sensation of change was expected. The effect of the rate influence is as marked for these observers as for the others and their mean variation is not excessive. This argues that, since they had nothing but the direct sensation to judge by, the real signs of change must have been present in a much larger proportion of trials than the above percentages would indicate. The error cannot be explained as due to the ordinary premature automatic reaction, for then the mean variation should have been much larger.

The figures in the table express the law that, for the three rates investigated, the threshold of perceptible increase in lifted weights is higher in fast rates than in slow rates. This accords with the law we found for pressure. The relation of the two will be discussed later.

V. Threshold for instantaneous increase in weight.

Apparatus D; Experiments.

This problem requires an apparatus by which the standard weight of a body may be increased instantaneously without causing any other dis-

turbance. This was accomplished by a compound weight balance. The cell (C_1) and the arm rest (C_2) are used as in the foregoing series. A weight-pan is suspended by a silk cord, branching out from the center of the bottom of the cell, 400^{mm} below it. Midway between the cell and the weight-pan there is a light fibre balance beam 140^{mm} long with its fulcrum at the middle. It is adjusted with one extremity perpendicular over the pan and attached to it by a cord which passes between the branching cords and is so fastened to the pan that changing the support of the pan from this cord to the branching cords, or the reverse, will not cause the pan to shake. At the other end a hook is suspended by a cord 200^{mm} long. Upon this a series of gram weights was fitted to be hooked firmly. The hook is light ($\frac{1}{2}^{g}$) and makes the weights conveniently interchangeable. At first the instantaneous increase was made by means of a weight acting over a pulley by a cord from the hook end of the balance beam. This weight was allowed to drop 100^{mm} with the cord slackened 90^{mm}; thus the hook end of the balance would be elevated 10^{mm} at a definable rate. But the present method required that the change should be made noiselessly; therefore at the risk of some irregularity, I simply raised the weight by pulling a cord perpendicularly from the hook as quickly as possible.

The plan of the apparatus may be explained better by an example. Suppose we want to get an increase of 1^{g} on 40^{g} lifted by the cell. The cell weighs 25^{g} and enough weight is added to the pan to make it weigh 15^{g}. Since the 15^{g} are supported by the cell its total weight is 40^{g}. We then place the 1^{g} increment in the pan and counterbalance it by 1^{g} on the hook (including the weight of the hook); the two weights on the balance, therefore, do not act upon the weight of the cell. The observer grasps the cell and lifts it 2^{mm}; this makes the balance beam stand in a horizontal position. The grasp is made in such a manner that the whole side of the hand rests upon the support in a position that can easily be retained during a prolonged experiment. He is then lifting the standard weight, but when the hook-weight is raised suddenly its counterbalance is transferred to the support of the pan without any movement of the balance beam. I was able to elevate the hook-weight as quickly as it would start to fall by its own gravity, or faster; therefore, the increment was transferred to the new support at the rate that it would assume by its own weight when beginning to fall, i. e., the increase was made by simply releasing the support of the required amount without imparting motion to the standard weight. By this method any instantaneous increase may be made without impact. It is evident that by reversing the action of he balance a decrease in weight may be made equally well.

This series of experiments was made on the same observers as the fourth series, and under similar circumstances, in order that the two sets of results might be comparable. They were made during the same period, this experiment being made alternately before and after the other. The point to be determined here was the threshold for the perception of a so-called instantaneous increase in the weight of a body lifted by the rested hand. A form of the method of minimum variation was employed. The steps varied by one gram each and were taken alternately ascending and descending the series. In ascending, the steps of change were continued until the observer had perceived the increment correctly three times in succession; and, in descending, the series of steps was begun with the highest one of the ascending series or by one above it if this series was low. At a signal the observer lifted the cell to the 2^{mm} limit and as soon as steadiness was attained in this position another signal warned him to watch for an increase, to which he should react by saying "up" at the moment he perceived it. The change was made from two to six seconds after this signal. The observer's ability to react just at the right time was considered a criterion for his certainty and accuracy. The time of making the change was varied irregularly within this region of four seconds. If he reacted perceptibly before or after the change, due allowance for the reaction-time being made, the fact was recorded as an error or illusion. Of course, failure to react indicated that the stimulus was below the threshold. This time-criterion was chosen in preference to the method of control trials in order to make the experiment short. Instead of concentrating the attention on the moment two seconds after the signal, it here had to be scattered over a time of four seconds. This, presumably, tended to raise the threshold. Upon a definite inquiry each observer testified that he had not perceived any suggestion as to the moment of change except by direct feeling of change in weight.

The results are contained in Table V; the initials of the observers will aid in the comparison of the individual records here with those in Table IV.

The Δ value marks an arbitrary limit. With careful observers it may be considered a pretty safe limit, denoting the point above which we may expect to find all increments perceptible under similar conditions. The Δ value must be interpreted with reference to the mean variations and the figures in the first sections of the table. According to the table 4.7^{g} would be perceptible ahout seven times out of ten, or 70 per cent. of the trials. This percentage is brought down so low because it is an average for different observers and not for successive trials on the same observer. The last column gives the number of times each observer reacted wrongly.

In comparing this and the preceding table we notice the striking coincidence that the threshold for instantaneous change is the same as the threshold for the slowest gradual change. Between those two points the curve changes to nearly four times that height (see Figure 3). Here the relation between the instantaneous and the very slow increments is very different from the corresponding relations for pressure, where the instantaneous increment is only about one-ninth of the slowest.

TABLE V.

Threshold of instantaneous increase in a lifted weight of 40g.

	1g	2g	3g	4g	5g	6g	7g	Δ	d	ε
J. L.	1	4	6	4	8	10		4.6	1.1	1
V. S.	0	0	6	8	10			3.6	0.7	0
M. M.	0	0	1	7	10			4.2	0.4	0
J. P.	0	0	0	2	4	10		5.6	0.5	0
W. J.	1	7	5	9	10			3.4	0.8	3
A. S.	0	0	0	2	6	5	10	6.2	0.8	2
F. K.	0	0	1	2	9	10		4.9	0.4	0
E. J.	0	0	0	3	8	10		5.0	0.4	0
C. C.	1	2	1	4	8	10		4.9	0.5	5
	0.3	1.4	2.2	4.6	8.1	9.4	10.0	4.7	0.6	

The unit of measurement is the gram.

The number of complete determinations of the threshold for each observer is $n = 10$.

The increments are denoted by the numbers at the heads of the columns.

The numbers below these show how many times, out of ten possible, each was perceived.

Δ, the average increment above which the next two are correctly perceived, hence the threshold.

d, mean variation; the mean variation for the series is found by dividing each of these by $\sqrt{n} = 3.2$.

ε, total number of errors.

Experiments were made upon six observers to study the effect of delay of the stimulus. Instead of devoting a special section to them I will make a brief statement here.

Using the identical apparatus described above, I found how the threshold varied if the stimulus was withheld after the signal for the number of seconds that the averages in series IV indicate, namely, 2.5s, 9.6s and 26.1s, i. e., the time it required to perceive the change at the three rates of gradual increase. Ten trials were made on each of the four increments: 3g, 4g, 5g and 6g. The results may be stated in a general way in terms of the percentage of those increments which were perceived. These are: for 2.5s, 63%; for 9.6s, 69%; and for 26.1s, 67%. The difference is not large enough to indicate any tendency toward a systematic variation.

The elements of fatigue and distribution of attention spoken of in the

second series seem to counterbalance each other here. Thus after the 26.1^s delay the standard seems lighter on account of the fatigue; but an instantaneous increase at that point will not be affected by fatigue, and will, therefore, appear larger in proportion to the standard at that point than at any previous point. On the other hand, at the end of 2.5^s there is no noticeable effect of fatigue, but the attention is not yet so strongly focused.

The present experiments should be compared with those in series II. Here the change was instantaneous, there gradual. The difference in the results points to the fact that the methods of perceiving change are entirely different in the two cases.

VI. Verification of series I and IV.

To increase the data found in series IV and to verify the law expressed in the results of series I and IV by means of a different method, the latter series of experiments was repeated by a form of the method of minimum variation. The only alteration necessary in apparatus *C* was to insert a scale reading in grams instead of the millimeter scale on the graduated tube. The steps of increase in the experiment differed by two grams each, running from 2^g to 20^g.

I tried different steps at random and considered a determination complete when I had found at least three consecutive steps in which the increment had been correctly perceived and all the steps below this had been tried. I was fully aware of the influence this procedure has upon the mean variation, but it secured a good distribution of attention and served as a sort of control method in that the observer had no means of knowing whether the increment should be a large or a small one as he would if the threshold had been approached by steps taken in regular order. If in the large steps the observer was sure that he felt an increase before the full amount had been reached, he was allowed to signify this in order to save time, but such a reaction was recorded as if only the whole increment had been perceived. No steps higher than 20^g were tried. Five complete determinations were made for each rate by each observer.

At the signal the observer lifted the 40^g cell to the standard position, and at another signal he began to watch for an increase in weight which began two seconds later except in the control trials. A final signal was given at the end of the increment and upon this the observer had to reply immediately by one of three answers, namely: "change," "no change," or "uncertain."

The method of manipulating the apparatus may be understood from the explanation in series IV; the main difference was that here definite amounts of increase were produced and the observer stated whether he perceived them or not, while there the change continued until he reacted.

The possible inaccuracy in reaching the exact increment in the fastest rate was $\pm$ 0.2[g]. If a larger error than that was made the trial was repeated. The variation by unsteadiness of the observer's hand may introduce a possible error of $\pm$ 0.2[g]. These are the only two marked sources of error. The first affects only the fastest rate.

The results for the observers are contained in Table VI. The first part of the table shows the numbers of R (change perceived) and U (uncertain) answers that were given out of five trials on each increment. At the right hand end of each line I have omitted all but the first "5" when this is the first of three successive fives. The R and the U trials are recorded separately and the reader may distribute the U trials as he thinks best. A general expression of the results may be gotten by a study of the R trials alone.

TABLE VI.

Threshold of increase in a lifted weight of 40[g].

			2[g]	4[g]	6[g]	8[g]	10[g]	12[g]	14[g]	16[g]	18[g]	20[g]	Δ	a
M. M.	I	R	3	5									2.8	1.0
		U	2											
	II	R		1	3	4	4	5					7.2	1.0
		U		2	2									
	III	R			1	3	3	5					10.4	1.2
		U	1	3	2		1							
F. C.	I	R	3	5	3	5							5.4	2.2
		U												
	II	R		1	5								5.6	0.6
		U		2										
	III	R		1	2	3	5						8.4	1.2
		U		1		2								
N. H.	I	R	4	4	4	5							4.0	2.4
		U												
	II	R		2	5	5	4	5					5.2	1.0
		U												
	III	R			2	3	5						8.0	0.8
		U				1								
B. L.	I	R	1	3	5								4.4	1.2
		U	1											
	II	R	1	2		4	4	4	5				9.2	1.9
		U			3	1								
	III	R			1	3	4	1	5				12.4	2.6
		U						2						

J. P.	I	R		1	3	3	5						7.6	1.9
		U												
	II	R				3	3	4	5				10.0	2.4
		U												
	III	R					2	2	1	1	3	4	16.8	3.0
		U					1		1					
W. J.	I	R	2	3	2	5							6.4	1.9
		U			1									
	II	R			2	4	4	4	5				8.4	2.4
		U			1									
	III	R				2	3	4	4	4	4	4	12.0	3.2
		U			1	1	1		1					
J. R.	I	R	2	2	4	4	4	5					6.4	2.2
		U	2			1								
	II	R		3	4	3	4	5					8.4	2.4
		U				1								
	III	R	1	1	1	2	3	3	4	4	5		12.0	3.2
		U			2									
W. H.	I	R	2	4	5								4.0	0.8
		U		2										
	II	R		4	4	5							5.4	1.5
		U		1	1									
	III	R	1		4	4	4	5					7.6	1.9
		U		2	1									
B. H.	I	R	3	4	5								3.6	1.3
		U												
	II	R	1	3	5	2	2	5					10.0	2.4
		U					1							
	III	R			1	4	3	1	5				13.2	1.3
		U												
R. S.	I	R	5	3	4	5							3.6	1.9
		U		1	1									
	II	R		4	1	5	4	5					8.4	1.4
		U			1									
	III	R			1	2	4	4	5				11.2	1.4
		U		1	3									
Av. % of R	I		50	68	80	94	98	100					4.8	1.7
	II		4	40	58	80	78	94	100				7.6	1.7
	III		4	4	26	52	72	70	78	88	94	96	11.2	2.0

The unit of measurement is the gram.
Number of trials on each point, $n = 10$.
Roman numerals give the rates:

I, 0.18^g per sec.
II, 1.10^g " "
III, 6.63^g " "

R, number of times the reply "change" was given in five trials.

U, number of times the reply "uncertain" was given in five trials.

The difference between 5 and R + U will give the number of replies "no change."

The numbers at the head of the columns denote the increments.

Δ, the threshold above which it is probable that all increments would be perceived.

d, mean variation of Δ; to find the mean variation for the series divide by $\sqrt{n} = 2.2$.

I have here assumed a standard condition which gives a threshold of the same degree of probability in all rates. This is obtained by the conditions upon which the figures of the Δ column in the second part of the table are based, i. e., the first of these consecutive increments that has been correctly perceived is taken for the threshold. With this as a standard, we may compare the rates with each other and, with certain precautions, the general results with those obtained by the reaction method.

There is a remarkable uniformity in the results, considering the delicacy

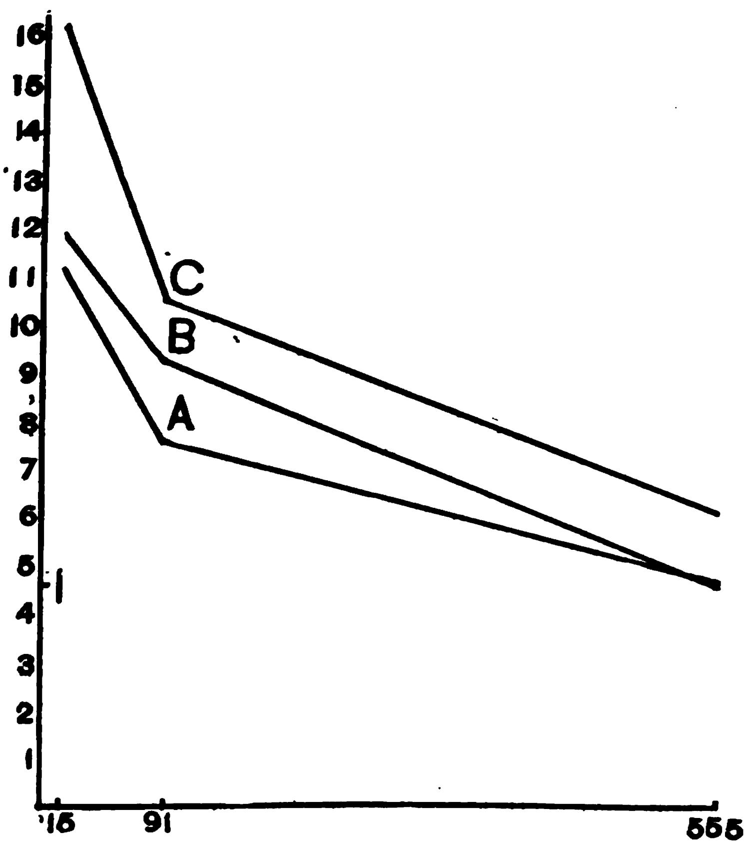

FIG. 3.

A, Results in Table IV.
B, Results in Table VII.
C, Results in Table VI.

The horizontal axis is divided into parts proportioned to the number of grams of increase per second, or, which is the same, to the part per second of the initial stimulus by which the change was made. The increments are laid off in grams as ordinates.

and difficulty of this discrimination and the lack of practice. Bearing in mind the differences in the conditions of these experiments and those

TABLE VII.

Five experiments on the author under conditions similar to those in the preceding table.

			4s	6s	8s	10s	12s	14s	16s	18s	Δ	d
A	I	R	1	2	4	5					5.6	0.8
		U	1	2	1							
	II	R				3	4	5			10.8	1.7
		U	1		4	1	1					
	III	R				1	3	5	1	5	15.6	1.8
		U			1	1	1	0	2			
B	I	R	1	3	5	4	4	5			8.0	2.4
		U	1			1						
	II	R			1	2	3	5			11.6	1.4
		U		1	3	2						
	III	R					2	3	5		14.4	1.7
		U		1	3	3	3	2				
C	I	R		4	5						6.4	0.6
		U	4	1								
	II	R			3	5					8.8	1.0
		U	1	2	1							
	III	R			2	2	5				11.6	2.0
		U			2	2						
D	I	R	1	4	4	5					6.0	0.4
		U	2	1								
	II	R			3	5					8.8	1.0
		U		2	1							
	III	R		1	3	4	5				9.2	1.8
		U										
E	I	R	3	4	4	5					5.6	1.9
		U	1	1	1							
	II	R		5							6.0	0.0
		U	1									
	III	R			3	5					8.8	1.0
		U		1								
Ave. % of R	I		24	68	88	96	96	100			6.3	0.7
	II			12	80	88	100				9.2	1.6
	III			4	32	72	80	92	84	100	11.9	2.5

Capitals denote the successive experiments. | Other notation same as in Table VI.

in series IV we may obtain some general conclusions from this comparison:

1. The law of the relation of the increment to the rate of increase established by the previous method in series I and IV is supported and

proved beyond the possibility of a doubt. This applies to series I only indirectly, but the general agreement between the facts and conditions of series I and series IV justifies the deduction in regard to the qualitative statement of the law.

2. The slowest rate requires the same increment, within 0.1^s, by this as by the previous method. By the present method the other two rates require smaller increments, somewhat in proportion to the rates.

3. The proportional differences by the two methods do not point to any notable error in the previous method, but are a good expression for the difference in the mental attitude in the two methods.

The foregoing experiment was repeated five times upon the writer during as many successive days. The conditions differ in that I knew just what was going on physically except in one respect, namely, that I did not know whether the trial would be a regular or a control trial. In this series I do not record the result of the control trials because I set my standard so high that I made practically no error. The following conclusions may be drawn from the summary in Table VII.

1. The general direction of the rate-influence is the same as that found for the other observers.

2. There is a noticeable decrease in the rate-influence for the fastest rate during the progress of the experiment.

3. A reasonable expression for the difference in mental attitude due to my knowledge of the rate of change is found in the amount by which my threshold differs from that of the other observers.

VII. Effect of the Rate of Change upon the Perception of Very Rapid Increase in Weight.

Apparatus E.

Very rapid rates of change being desired here, it was necessary to use a float of larger diameter, or nozzles allowing a more rapid stream to flow, than had hitherto been used. The former alternative was adopted, but with this alternative a change in the mechanism of the rest of the apparatus also became necessary. Apparatus *C* required that the operator should stop the increase when it had reached the desired height. The rates of change approaching the instantaneous, here required, could only be obtained by an automatic determination of the amount of increase by the apparatus itself. This was accomplished as follows:

Apparatus *C* was used in all its parts except three, namely: the float, the connection between the cell and the beam, and the rod which counter-

balanced the float. A light steel tube of 25^{mm} diameter was used for a float. The connection with the cell was reëstablished in the following manner. A balance beam E of the same dimensions as the balance beam A is placed immediately above A and parallel to it. We may name the float end of the original beam Af, and the corresponding end of the upper beam Ef and the other ends respectively Aw and Ew. Ef is vertically over Af and they are joined by two loops of fine wire which interlock at the middle in such a way that when the tension is released and Af and Ef approach each other the loops cause no friction and allow no spring or elasticity; the lower is fixed in an upright position and the upper falls vertically by its own weight. Now, following the same principle as in apparatus D, the standard weight is maintained by means of the compound balance action. The cell is attached to Ew and from the same point a balance pan (pan E) is suspended 400^{mm} below. In place of the rod at Aw another scale-pan (pan A) is suspended at the same distance as pan E. When the weight in pan E is equal to the weight of the float at zero the two beams stand in a horizontal position and the loops between Af and Ef are just on the point of making contact. A is balanced and exerts no influence on E, and E is balanced and exerts no influence upon the cell which, when lifted, has its own weight plus the weight of pan E. If the weight in pan A be diminished Af will pull on Ef by that amount and this will in turn lift at Ew, which lessens the weight of the cell by the same amount.

The balancing of the apparatus depends upon the position of the cell when lifted. A special support for the cell regulates this. It consists of a trap on top of the arm support (C_1). This is essentially a slat with one end hinged to the rear end of the arm support. Its front end is held up against an adjustable catch by a weight acting through a cord over a pulley. This cell is placed on the front end of this slat in the same lateral position as before, but it is supported at the standard height to which it should be lifted. When the cell is grasped it, is held in the same comfortable position. The experimenter elevates the weight which supports the trap, the trap falls and leaves the cell supported by the hand in a definite position.

Let me further explain the apparatus by an illustration of how it works. Suppose I desire to produce a measured increase of 2^g on a standard of 40^g at a very rapid rate. The cell itself weighs 25^g and a weight is placed in pan E, so that the total weight of the cell when held in position, after the trap is dropped, will be 40^g. Both beams are balanced in a horizontal position and exert no influence on the cell. Then we take off 2^g from pan A. This makes Af pull on Ef

by 2^g and this in turn lifts the pan E by 2^g, reducing the standard to 38^g. This we correct by adding 2^g to pan E and again obtain our standard, 40^g. But now pan E is lifting 2^g at Ef and the same amount at Ew; if we then gradually restore the balance of A by immersing the float, E being stationary, the amount lifted at Ew will decrease until the zero point is reached and the contact between Af and Ef is broken. The contact is broken at the moment 2^g have been added to the weight of the cell making it 42^g. The rule determining the size of the increment is that the desired weight must be removed from pan A and placed in pan E. The rate of increase is determined by the size of the stream of water as before.

The above is an entirely satisfactory solution of the problem of how to produce quick and accurate changes of weight in a given standard without movement or jarring of the body lifted. The adjustment to the zero point may permit a possible error of $\pm \frac{1}{8}^g$. The trap holds the cell in the correct position, and, after a brief practice, the observer can retain this approximately, but for accidental movements of the hand there may be an error of within $\pm \frac{1}{2}^g$. There could be no other inaccuracy in the size of the increments, since they were made by actual interchange of gram weights.

The rates were timed by the graphic method of recording time. The recording pointer was placed in circuit with a means of making the circuit at the beginning of the increment and breaking it at the completion. The electric clamp key was used as in series I to make the circuit the moment the stream was let on. A platinum contact was built up at Af, such that Af rested upon it and kept the circuit closed as long as Af was heavier than Aw. The moment the beam A passed through its point of balance and the tension on the interlocking loops was released, Af left its contact and the electric current was interrupted.

I first measured the time of all the increments (the steps in the series differed by 2^g each) from 2^g to 20^g on one rate to find if the changes were uniform. This rate made the change at the rate of 1^g in 0.12^s. Taking the time to increase 20^g as a standard, and calculating the theoretical time for each increment, I found that the empirical results deviated from the theoretical only by an irregular fluctuation no larger than might be allowed for the error of measurement, i. e., there was no systematic error large enough to demand consideration here, and the increase may be considered as practically constant. Having found this I timed all the rates for the step of 20^g, and divided that up proportionately for the other steps. The rates adopted and measured in this manner are as follows:

	I.	II.	III.	IV.	V.
α	4.54	8.33	33.33	50.00	66.66
β	0.22	0.12	0.03	0.02	0.015
γ	0.11	0.21	0.83	1.21	1.54

Here α denotes the number of grams of increase per second, β the number of seconds to increase one gram, and γ the part of the initial stimulus per second.

Experiments.

The fastest rate hitherto used required the largest increment. Referring to the curves in Figure 3, what is the highest limit for these if they be continued to the left, and what form will they assume in returning to the low point that marks the instantaneous increase? This is the question I have tried to answer by the present series of experiments.

The quoted rates are within the limits of sufficiently accurate measurement by the present apparatus. The slowest connects with the fastest of the previous and the fastest approaches the instantaneous.

The method of minimum variation was here used somewhat differently from the previous manner. The threshold was determined five times for each rate as follows: the increments differed by 2^g each, but, in order to save time, five trials on each step were made in succession. A number of steps in the middle region were tried until a block of records was obtained in which the change had been perceived correctly every time in the largest of the increments tried and no time in the smallest. The few exceptions to this rule may be seen in the records from the fact that the highest number is less than five in those cases.

The observer was given a choice of two answers only, namely, "Change" or "No change." The merits and demerits of that limitation are well known. At a signal the observer grasped the cell with the hand in position to rest firmly. At a second signal the trap fell and he was to look for the differentiation which might begin about two seconds afterwards. He was required to give his answer as soon as the increment was completed.

The results for six observers are contained in Table VIII. The left hand section of the table shows the number of perceived changes out of five possible for each step. There the variation of any single increment may be traced for each observer. All but the first of the successive "5s" are omitted, and all increments above this are counted as perceptible in the respective single determinations of the threshold. Δ is found as before by taking the average of the single thresholds above

TABLE VIII.

Threshold of increase in a 40g weight at five rapid rates.

		2g	4g	6g	8g	10g	12g	14g	16g	ε	Δ	d
F. C.	I		1	5						0	4.8	0.8
	II			5	5	4	5			0	7.2	1.8
	III	1	3	3	5					0	5.0	1.8
	IV		3	4	5					0	5.2	1.4
	V	1	2	5						0	5.2	1.0
N. H.	I			1	2	0	5			0	12.0	0
	II						2	2	5	0	14.8	1.4
	III				3	2	2	5		0	12.4	1.6
	IV			1	4	5				0	9.2	1.0
	V			4	0	2	5			0	10.8	1.4
V. S.	I		1	1	3	3	4	3		0	12.4	2.0
	II		1	3	3	4	3	5		0	11.2	2.6
	III		1	4	5					0	6.0	0.8
	IV	3	3	4	4					1/4	6.4	2.0
	V		4	2	3	5				0	7.6	2.0
W. J.	I		2	2	5					1/3	6.9	1.4
	II		3	3	5					1/4	6.0	1.6
	III	4	4							1/2	2.8	1.4
	IV	3	5							1/2	2.8	1.0
	V	5	5							1/2	2.0	0
J. R.	I		2	2	3	2	3	3	5	0	14.0	1.6
	II		1	2	3	3	4			1/5	11.6	1.2
	III	4	4	3	5					1/4	4.4	2.8
	IV	4	2	4	4					1/4	5.2	2.6
	V	4	5							0	2.4	0.4
W. H.	I		3	3	5					2/4	5.6	2.0
	II				3	5				0	8.8	1.0
	III			2	1	2	5			0	11.2	1.0
	IV		1	2	2	5				0	9.2	1.0
	V	1	2	4	4	3	4			0	9.6	2.4
Av. %	I	0	30	47	77	90	87	100		3/17	9.3	3.5
	II	0	27	43	63	70	80	90	100	2/18	9.8	2.6
	III	30	40	57	77	80	90	100		2/23	7.0	3.3
	IV	33	47	67	80	100				3/21	6.3	1.9
	V	37	60	83	90	83	97	100		1/23	6.3	3.1

Roman numerals, rates of change.

Figures at the top, increments in grams.

ε, the numerator gives the number of errors that were made in the number of control trials denoted by the denominator.

Δ, threshold.

d, mean variation: to find the mean variation for the series divide each by 2.2.

The average is stated as the percentage of the possible number of correct answers.

which all increments were perceived, on the supposition that, when an increment had been perceived every time, all steps above that would also be perceived. The assumption is quite valid, for the steps are larger and the successive steps would be taken under similar conditions with reference to the larger fluctuations in sensibility. The value of this threshold must also be estimated by its mean variation and by a comparison with the distribution of the figures in the first part of the table.

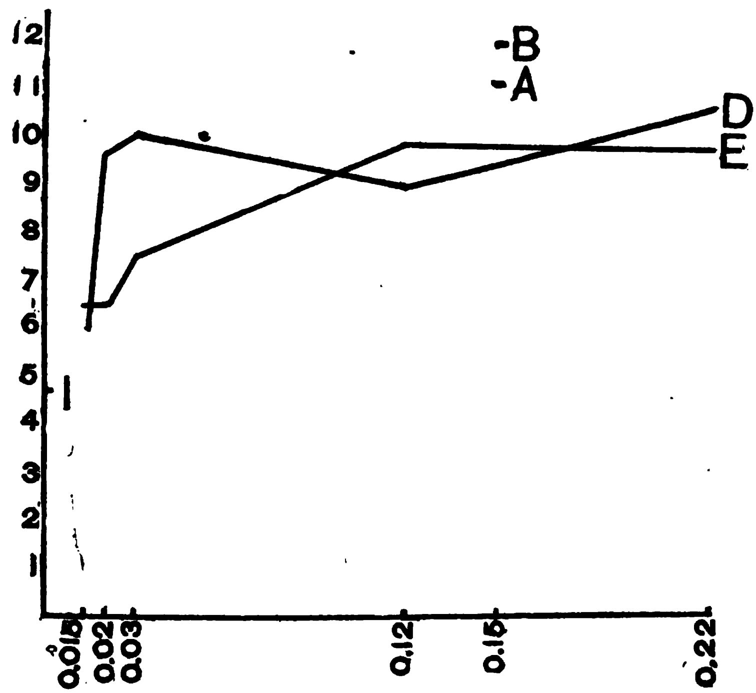

Fig. 4.

E, Results in Table VIII, p. 56.
D, Results in Table IX, p. 58.
I, Results in Table V, p. 46.
A, Highest point in *A*, Figure 3, p. 50.
B, Highest point in *B*, Figure 3.
The horizontal axis is divided on a scale 20 times as large as in Figure 3.

The number of experiments is too small to afford a detailed expression of the law of variation, but it is evident that we have found the maximum or turning point in the size of increments. The results as expressed in curve *E*, Figure 4, show the gradual return to the point of instantaneous increase. This curve shows their relation to the point *I* denoting instan-

taneous increase and the point *A* denoting the highest point in curve *A*, Figure 3, i. e., the threshold for the rate of 1^s in 0.15^s. The object of these experiments is to trace the connection between *H* and *I*. Since their relative distance is very much magnified in Figure 4, the scale in this figure should be compared with the scale in Figure 3.

The above experiment was repeated four times upon the writer, as far as possible under similar conditions. The results are expressed in Table IX and curve *D*, Figure 4.

TABLE IX.

Four experiments upon the author; conditions similar to those in the preceding table.

		2^s	4^s	6^s	8^s	10^s	12^s	14^s	e	Δ	d
A	I	2	2	4	5				1/4	5.6	1.2
	II			2	5				1/3	7.2	1.0
	III			3	3	3	5		0	10.4	1.2
	IV	1	2	2	5				1/4	6.8	1.4
	V	1	3	4	5				0	5.2	1.8
B	I				2	4			1/3	9.6	1.2
	II				3	5			0	8.4	1.2
	III			1	2	5			0	9.2	1.0
	IV			5					0	6.0	0
	V		2	4					0	6.0	0.8
C	I				1	2	3	2	1/5	14.0	2.4
	II			1	1	5			0	9.6	0.4
	III			1	3	2	5		0	11.2	1.0
	IV			3	4	5			2/4	7.2	1.4
	V		4	4	4				2/4	6.0	2.2
D	I			2	2	4	3		0	14.0	1.6
	II			1	2	4	4		0	10.8	1.8
	III			2	3	3	5		0	9.2	2.2
	IV		1	3	4	4			1/4	7.6	2.0
	V		2	3	5				0	6.4	1.2
Ave.	I	10	10	30	50	75	80	85	3/16	10.8	2.7
	II	0	0	20	55	95	95	100	1/14	9.0	1.2
	III	0	0	35	55	65	100		0	10.0	0.8
	IV	5	10	65	90	95	100		4/14	9.6	0.5
	V	5	55	75	95	100			2/15	5.9	0.4

Notation same as in Table VIII.

These results agree with the foregoing in that they show that the threshold is lowered as the point of instantaneous increase is approached.

In comparing the superposed curves in Figures 3 and 4 it must be remembered that there are important differences in the conditions upon which the results in each curve are based. Therefore, the comparison must not be one of absolute units, but of general directions and tenden-

cies. Thus, in Figure 3, the curves descend in a decided manner towards the right, and in Figure 4 they descend toward the left, though not with as great regularity. The curves in these two figures may be joined together and then they will express the general law stated in the next paragraph.

VIII. Some observations and conclusions.

The various conclusions in regard to the influence of the rate of change upon the threshold of change for pressure and muscle sense made under the conditions adopted in the above experiments may be generalized as follows: the threshold for the perception of difference in pressure and lifted weight rises rapidly from the threshold of instantaneous change and soon reaches a maximum from which it falls off gradually until the slowest rate at which the change can be perceived at all has been reached.

This is not an absolute law, but it is a well defined tendency. Its value depends largely upon the conformity of the above conditions of experiment to the normal conditions of our every-day experience. I have hoped to obtain the facts undistorted by using the so-called "unconscious" method and making the results partially statistical. I am not going to enter into any polemic with those who have found contradictory results (mainly Stratton), for, like other factors in our perception, the rate influence depends upon its relation to a number of unknown subjective and objective conditions which determine its nature and effect. The subject has just been opened for experiment.

Any law expressing the influence of the rate of change upon the perception of difference in sensory stimuli must be stated particularly with reference to the following among other factors:

1. The special sense organ. The law, derived from the above experiments, is indicative of relations that we find to obtain in other senses. Thus, there is a general agreement between these results and those found by Scripture and Stern on sight and by the same authors on sound (see references, p. 27), but there are important differences depending upon the functioning of the particular sense organs. The above may be made a general law of sensation, but it has definable peculiarities for each sense. Compare, e. g., Stern's results on sight and sound or mine on pressure and weight.

2. The kind of threshold. The rate influence has mainly been studied in the threshold of change. It is equally important and may be just as well studied in other thresholds, e. g., the threshold of sensation. I have made some experiments upon the least perceptible touch as depending upon the rate of impact of the stimulus. These experiments were made with a modified form of apparatus *A*. The rate influence was here more

marked than that for the threshold of difference in pressure, i. e., the largest stimulus was required near the instantaneous rate, which was much lower than for any gradual stimulus. At very slow rates the perception became very uncertain. Thus, I found it possible to apply a pressure of 4^g over an area only 1^{mm} in diameter upon a finger without the observer being able to detect it. I have made similar observations on the thresholds of sight and sound and on the thresholds of sensation, disagreeableness, and pain under electrical stimulation. These last experiments were made by an ordinary slide inductorium and a pair of electrodes. In such experiments the psychological method promises to be of great value for the study of the development of æsthetical ideas and tastes as depending upon the rate at which the sensory impressions are made, e. g., in approaching the threshold of pleasure or pain. And, what is mainly of theoretical interest in psychology has a very extensive practical interest for education.

Hall and Donaldson and Stern (see references, p. 27) have studied the rate influences in the perception of motion by sight. I have found a marked variation in the perception of tactual and muscular space. It has been customary to take the rate variation into consideration in estimating lifted weights by requiring that the weights should always be lifted to the same height at the same rate. Müller and Schumann's[1] measurements on this point are valuable.

I have constructed a dynamometer for measuring active pressure. A beam 300^{mm} long is supported upon pivot bearings at one end. The other end carries a pointer which moves over an arc graduated empirically from 0^g to 1000^g. The pointer end of the beam is supported by a steel spring of seventy coils hung vertically. The pressure point is a hard rubber disk 15^{mm} in diameter supported by a loop from a point on the beam 75^{mm} from the bearing end of the beam. This is a convenient and satisfactory dynamometer. In some experiments I required the observer to press to the standard, 500^g, and then reproduce it from memory at various rates of increase in pressure so as to reach the standard in the following times: (1) 2^s; (2) the observer's own time, generally about 5^s; (3) 5^s; (4) 10^s; (5) 15^s; (6) 20^s; (7) 25^s. The standard was pressed before each single trial; it was aimed to reach it in 5^s. The results show that the slower the pressure increases the more it is overestimated. The 2^s pressure is generally underestimated.

What is true for one unit applies also more or less to other units of measurement in sensation. I made some experiments, e. g., on the double

[1] Müller and Schumann, *Ueber die psychologischen Grundlagen der Vergleichung gehobener Gewichte*, Archiv. f. d. ges. Physiol. (Pflüger), 1889 XLV 37.

stimulus in pressure with a standard of 5^g using apparatus A as in series I with rates I, II and V. The results for five observers were: I, 10.5^g; II, 12.1^g; and V, 13.4^g, i. e., the faster the increase the more the comparative pressure is underestimated. This is in accord with the laws found for dynamometry and for the threshold of pressure.

The rate influence is not limited to sensation. It enters our higher and more complex emotional and intellectual experience and activity.

3. The standard stimulus. The variation with the standard stimulus is, perhaps, best expressed by Weber's law.

4. The direction of change. For most stimuli there is a close relation between the threshold for increase and the threshold for decrease.

5. Method of marking the beginning and the end of change. Any method which leaves the reaction-time in the results is necessarily crude and the methods of elimination are uncertain. Introducing other methods changes fundamental factors in the conditions. STRATTON's experiments and the mine have shown that the results depend to a great extent upon the method. According to STRATTON, the law may even be reversed. We may grant that possibility, but that does not detract from the value of our results, for each method reveals a characteristic tendency. It seems to me that STRATTON's reversal of the law obtained by a gradation-method, is not due only to the change in method of recording the end of the increment, but to this in connection with other important factors, such as the special conditions of knowledge of the physical relations and the state of preparedness.

6. Knowledge of the facts. This changes the laws of perception in several ways. Knowledge of the physical facts acts as a suggestion. A conscious or unconscious distortion or correction is liable to creep in. The unconscious corrections are perhaps the most vitiating. Our ordinary experience affords us examples of change in which the physical processes are known and others in which they are not, and our experiences are different in the two cases. This is just what we find in experiment. From this point of view the cases in question must be stated with reference to (1) the state of expectancy or preparedness, (2) the distribution of attention, and (3) the degree of complexity of the discrimination. It is well known what a difference it makes whether a person knows what to expect and when and how to expect it. Such knowledge guides the distribution of attention. Thus, if I must distribute my attentive energy over 25^s it will be less potent at any moment of response than if it were sharply focused just for that moment. But on the other hand, during a prolonged uncertainty, expectation rises and the effort of attention becomes greater and greater.

WEBER'S LAW IN ILLUSIONS.

BY

C. E. SEASHORE, PH.D.

In a former article[1] I reported some measurements on illusions of weight. Since then it has occurred to me that we may not only measure an illusion, but also use this measurement as a means by which to determine some other mental factor whose relation to it is known. An experimental attempt at this subject revealed another problem which is involved in the first and, at the same time, affords a field in which the two may be solved together. This second problem, which, on account of the outcome of the experiments, proves to be the most important, is this: Does Weber's law depend upon the so-called real intensity or upon the apparent intensity of the stimulus?

As a most distinct and manageable case in which to carry on the investigation I selected the illusion of weight which is due to the knowledge of the size of the object lifted. The apparatus consisted of three pairs of cylinders (*A*, *B* and *C*) of the same weight, 80^{g}, and the same diameter, 37^{mm}; but of different lengths, *A* being 20^{mm}, *B* 120^{mm} and *C* 50^{mm}. The cylinders were made of polished hard rubber; in external appearance they were similar in all respects except length. The bottom plates were screwed in and could be removed by turning them three-fourths of a revolution by a key. A steel pin rose from the center of each bottom-plate for the purpose of receiving different weights consisting of circular disks with holes in the centers to fit this pin. There were two sets of these weights; one of 1^{g} to 15^{g} by one-gram steps and the other of 5^{g} to 40^{g} by five-gram steps. The adjustment of the weight by this method was suggested by Dr. MEUMANN, of Leipzig. It is a modification of Professor JASTROW's muscle sense apparatus.[2]

Twenty students in experimental psychology were examined and asked to give their judgments regarding the heaviness of the weights but to make no correction, allowance, or guess, based upon knowledge of the

[1] SEASHORE, *Measurements of illusions and hallucinations in normal life*, Stud. Yale Psych. Lab., 1895 III 1.

[2] I am under obligations to Professor BLISS for facilities and suggestions, and to members of his class in the New York University summer school, 1896, for assistance as observers in the present experiments.

illusion involved. As has been proven (pages 5–9 in my investigation cited above) the illusion of weight persists, but is not so strong when the fact of the illusion is known; therefore, observers who are aware of the illusion and those who are not aware of it fall into two distinct classes. In the present experiments even the details of the illusions of weight and of this illusion in particular were demonstrated before the observers until all were conversant with the facts in question. Therefore, the illusion here measured is not the maximum. The preparatory demonstration was, however, made by a different set of cylinders in which the diameters varied so that none of the observers knew the exact extent of the illusion in the present apparatus.

The aim was to determine two classes of facts: (1) the threshold, or least perceptible difference, for each pair of cylinders, and (2) the amount and kind of illusion in A and B respectively when measured by C as a standard. The first was determined by the following method: The weights in the continuous series rising by one gram each step were tried until three successive increments had been correctly perceived. The lowest of these was considered a threshold value. The observers were allowed to answer "equal" or "different" and, in the latter case, they were required to point out the heavier. The amount of the illusion was found by determining how much the weight in the C cylinder had to be varied from the standard in order to make it equal to the A cylinder. The same procedure was repeated for the B cylinder. The series of weights which differed by five-gram steps were used in the measurement of the illusion.

The results are given in the Table. One determination was made in the order ΔA, ΔB, ΔC, K, K' of which the results are recorded in the a-columns. Then the series was repeated in the reverse order with another complete determination on each point. These results are recorded in the b-columns. For the threshold a and b have the same value, but in the illusion-measurements a represents the lowest difference which made these two cylinders apparently equal, and b the highest; i. e., in a I started from the point of physical equality and continued to the first point of subjective or apparent equality, while in b I started with an excessive difference and decreased this until the upper limit of apparent equality was reached.

The experimenter handed the cylinders from behind a screen by pairs, placing them on end side by side in a convenient position upon a baize-covered table. The observer was required to grasp them as nearly as possible in the same manner and lift them always to the same height with the same speed. He was also required to keep them as near to-

gether as possible, and, after having tried them back and forth, to interchange them in position and again compare them in both directions, continuing as long as he thought profitable. Thus the errors of time, speed, place, fatigue, practice, surprise, temperature and order were fairly eliminated.

Subject.	ΔA			ΔB			ΔC			K		K'	
	a	*b*	*E*	*a*	*b*	*E*	*a*	*b*	*E*	*a*	*b*	*a*	*b*
I	4	1	$\frac{1}{9}$	4	3	$\frac{0}{11}$	1	2	$\frac{0}{7}$	10	20	10	20
II	5	3	$\frac{1}{12}$	7	5	$\frac{1}{16}$	4	4	$\frac{1}{12}$	15	15	20	15
III	3	2	$\frac{3}{9}$	3	2	$\frac{0}{9}$	1	2	$\frac{0}{9}$	15	15	15	15
IV	2	1	$\frac{1}{7}$	1	4	$\frac{0}{9}$	1	8	$\frac{0}{15}$	20	25	10	20
V	2	4	$\frac{2}{10}$	3	4	$\frac{0}{11}$	3	1	$\frac{1}{8}$	0	0	10	5
VI	1	1	$\frac{0}{6}$	3	1	$\frac{0}{8}$	3	3	$\frac{0}{10}$	20	20	15	15
VII	3	6	$\frac{3}{13}$	4	6	$\frac{0}{14}$	4	6	$\frac{2}{14}$	10	20	15	15
VIII	4	2	$\frac{0}{10}$	3	3	$\frac{0}{10}$	2	2	$\frac{0}{8}$	20	10	15	15
IX	1	2	$\frac{1}{7}$	6	2	$\frac{2}{12}$	2	7	$\frac{2}{13}$	15	10	10	5
X	3	4	$\frac{0}{11}$	5	8	$\frac{0}{17}$	6	9	$\frac{0}{19}$	20	25	10	20
XI	1	2	$\frac{0}{7}$	3	2	$\frac{0}{9}$	1	1	$\frac{0}{6}$	15	15	15	15
XII	3	1	$\frac{1}{8}$	4	4	$\frac{4}{12}$	3	3	$\frac{3}{10}$	15	10	5	5
XIII	3	3	$\frac{4}{10}$	1	6	$\frac{2}{11}$	3	4	$\frac{2}{11}$	20	15	0	5
XIV	2	2	$\frac{0}{8}$	2	2	$\frac{0}{8}$	3	1	$\frac{1}{8}$	25	25	10	15
XV	1	2	$\frac{1}{7}$	2	7	$\frac{2}{15}$	1	5	$\frac{1}{10}$	15	10	−5	5
XVI	7	1	$\frac{0}{12}$	7	5	$\frac{0}{16}$	2	4	$\frac{1}{10}$	10	20	10	5
XVII	1	2	$\frac{0}{7}$	1	2	$\frac{0}{7}$	3	3	$\frac{2}{10}$	15	20	15	20
XVIII	5	7	$\frac{0}{16}$	6	8	$\frac{0}{18}$	4	5	$\frac{0}{13}$	5	10	10	5
XIX	3	2	$\frac{0}{9}$	6	8	$\frac{1}{18}$	3	2	$\frac{0}{9}$	15	25	15	20
XX	3	6	$\frac{0}{13}$	2	7	$\frac{0}{13}$	4	9	$\frac{0}{17}$	10	25	20	20
A	2.9	2.7	$\frac{0.9}{9.6}$	3.6	4.4	$\frac{0.6}{12.1}$	2.7	3.9	$\frac{0.8}{10.9}$	14.5	17.2	11.2	13.0
d	1.1	1.3		1.4	1.7		1.1	2.0		4.2	5.5	4.4	5.6
M	2.8			4.0			3.3			15.8		12.1	
m	1.2			1.5			1.5			4.8		5.0	

The unit of measure is the gram.

ΔA, ΔB, ΔC, the thresholds for the respective pairs.

K, overestimation of A when measured by C.

K', underestimation of B when measured by C.

a, the first measurement.

b, the second measurement.

E, the proportion of errors made; the numerator expresses the number of so-called wrong judgments made out of the total number expressed by the denominator.

A, average; *M*, the mean of the two averages.

d, mean variation; *m*, the mean of the two mean variations. The mean variation for the series may be found by dividing these by 6.3.

The necessity of retaining the same standard throughout the entire experiment was urged upon each observer. The standard of sureness can be seen to some extent in the individual records by comparing the number of errors with the size of the increments. *E* shows the number of actual errors in comparison with the number of possible errors, or the total number of judgments that were made. The comparatively small number of errors in *B* may be accounted for partially by the favorable position of *B* in the order of trials. The *b*-trials in *C* were often disturbed by their proximity to the illusion-trials, and *A* was subject to still more disturbance because of its position as the extreme first and last trials. I call these so-called wrong judgments "errors" in a different sense from that in which the illusions are called errors. The illusions are normal, but what is here called an error is not based upon any such constant factor that is known.

The variation is apparently large, but it must be remembered that it is the individual variation of twenty different observers from their average and that the number of trials on each observer is small. The general agreement of so many is worth more than the consistency of one in a larger number of trials. If we consider the uniformity of the conditions of the observers, subjective as well as objective, we find ourselves justified in taking the averages of these twenty (in all, forty complete determinations on each point) as a fair expression of the answer to the two original questions.

Does Weber's law depend upon the real or upon the apparent weights? If upon the apparent, is there any traceable law? In the present case *A* is overestimated by 15.8^g and seems to weigh 95.8^g in terms of *C* which we assume as a standard and common measure. According to that a physical change of, e. g., 3.3^g in *A* will appear as a change of

$$\frac{95.8}{80} \times 3.3 = 4.0.$$

On the other hand *B* is underestimated by 12.1^g and appears to weigh 67.9^g in terms of *C*. Then a physical change of the same amount, 3.3^g, in *B* will appear as a change of $\frac{67.9}{80} \times 3.3 = 2.8$. There is a coincidence between this theoretical consideration and the above results, but we must seek other relations in the results in order to get a more direct answer.

The ratios for the relation between the respective thresholds and the standard for these particular conditions are:

$$\Delta B = \tfrac{1}{20}, \quad \Delta C = \tfrac{1}{24} \text{ and } \Delta A = \tfrac{1}{29}.$$

These are the constant multiples which would express Weber's law for the same conditions if the measurements were repeated with any standard weight within the limits in which the law is applicable. These are, however, only representative of a large number of possible illusions. As I have proved (pp. 3–5 in the article cited above) within certain limits, the illusion varies directly with the difference in size. We might have made the illusion stronger by making A smaller and B larger, or by obtaining the naïve, "unconcious" judgment of the observers. In such a case we should have gotten a number of thresholds outside of the present extremes. Similarly we might have decreased the illusion down to zero. It is evident then that if we state that Weber's law requires, e. g., $\frac{1}{24}$ of the standard to produce a just noticeable increment on 80^g we have stated it only for one particular relation between the standard weight and the size of the object. Grant that C is the nearest approach that we can get toward freedom from this one illusion, then the relation of the threshold to this would denote that which is generally expressed by Weber's law. But a large number of conditions, represented by A and B are just as important from a practical point of view, for it is very rare in normal life that we lift objects that give us no illusion. This was preëminently true in the classic experiments on Weber's law. Now, the facts known about the regularity and trend of this illusion justify us in assuming that, if the same proportions between the sizes and the standard weight be retained, the illusion will approach a constant fraction of the standard within the limits of the validity of Weber's law. Therefore, if we had determined the threshold empirically under all possible degrees of illusions like the one under discussion, Weber's law might be expressed for all standard weights within the above assumed limits by as many fractions, like $\frac{1}{24}$, as there are illusory effects such as A and B represent. The fact of this possibility implies an affirmative reply to our first inquiry, namely, that we can use the measurement of one mental characteristic as an index to another; in this case the measured illusion is an index to the threshold. An attempt to state it from such a complexity of conditions might seem hopeless, but the above results give us a key.

First, all overestimation lowers the threshold and all underestimation raises it. Secondly, retaining the notation of the Table, we may formulate the results thus:

$$\frac{\Delta A}{\Delta C} = \frac{C-K}{C}$$

and

$$\frac{\Delta B}{\Delta C} = \frac{C+K'}{C}.$$

The error involved by substituting the empirical results is 5% in the first equation and 10% in the second. Both these, it may be seen from the table, are less than the respective mean variations. Disregarding the illusion (K and K' in the above formulas) as has always been done, the error in substitution would be 15% in the first and 21% in the second equation. Hence Weber's law stands in a much closer relation to the apparent weight than to the physical standard.

If we know the increment needed for an object of a given weight and size, e. g., C, at any standard, and know the amount of the illusion for all differences in size in objects of this weight, we may be able to calculate the threshold of perceptible difference for all such cases. For

$$\frac{\Delta A}{C-K} = \frac{1}{28}$$

$$\frac{\Delta B}{C+K'} = \frac{1}{28}$$

and

$$\frac{\Delta C}{C} = \frac{1}{24}\,.$$

These give a constant fraction for all the equations, in this case approximately $\frac{1}{28}$. This fraction may be supposed to hold as a constant for all conditions, of which the above A, B and C are representative. Hence, we may state the principle for the dependence of Weber's law upon apparent weight, as

$$\frac{\Delta E}{S+K} = M$$

where ΔE is the threshold of perceptible difference, S the physical standard weight, K the amount of the illusion (which must retain its sign + or — according as it is underestimation or overestimation of the standard), and M a constant.

A question may here be raised as to the reason for and effect of choosing C of this particular size. It was chosen, after some preliminary experiments, because it seemed to correspond fairly to the size that the adopted standard weight might suggest. What would have been the effect of making it larger or smaller than this? It may be possible to detect some law for the dependence of the illusion upon this, but at the present stage of measurements in illusions this factor is negligible and the standard may be chosen of any size which is not so extreme as to introduce other sources of error, such as difficulty in grasping, provided the results are stated in terms of that size. Such results may also be convertible into

terms of each other, for on this theory we may add the illusion of A to the illusion of B and take the sum of these as the expression of the illusion of either A or B in terms of the other. Thus

$$\frac{\Delta A}{\Delta B} = \frac{B-(K+K')}{B}$$

$$\text{and } \frac{\Delta B}{\Delta A} = \frac{A+(K+K')}{A}.$$

The error involved by substituting the actual figures is 5% for the first equation and 7% for the second. Although these are extreme cases the error of the substitution is no greater than that found when the mean, C, was used as a standard. Therefore, within obvious limits, we are justified in choosing the standard of any convenient size in measurements like these.

Just as no one now claims an exact mathematical conformity for Weber's law in any sense, we must construe the above formula liberally. There may be some more determinable factors that must be taken into it; we can never hope to determine and control all such factors. Judgments as to the validity of the law have heretofore been made largely upon experiments that involved the illusion here discussed, or similar ones, and the variations caused by them have been counted as discrepancies in the law. The above data at least justify us in assuming this relation of Weber's law to illusions as a working hypothesis. It promises not only the same degree of comformity as the law has had on the old theory, but also an extension of it both in the degree of conformity and the range of its applicability.

RESEARCHES ON VOLUNTARY EFFORT.

BY

E. W. SCRIPTURE.

We may suppose that in primitive times quantities were measured according to mental scales; thus distances would be established by the eye or paced off by walking, weights would be judged by the effort required to lift them, etc. In most cases later civilization has, by the method of maximum agreement,[1] established successively finer methods wherein the disagreements due to the personal and instrumental differences are reduced to extremely small quantities. These are the so-called "physical" or "instrumental" methods. Thus by instrumental methods a scale for space is developed from the standard meter bar, or a scale of mass from the standard kilogram. With such scales in our possession the problem arises: how do our mental scales compare with the instrumental ones? The solution of this problem in regard to the voluntary efforts used in compressing the fingers was attempted in the following way.

APPARATUS.

The dynamometer. Experience with various dynamometers led to the construction of a new one. Two spring-steel rods are inserted into a brass block so that they extend from one side. Flat hard-rubber knobs are fastened at the appropriate distance from the block; when pressure is applied to these knobs, the rods bend inward after the manner of sheep-shears. A light plate is attached to the end of one rod; the end of the other rod is pointed to serve as an index. The amount of the pressure exerted on the knobs is measured by the deflection of the rods, and this is indicated by the distance through which the index passes over the plate. The physical scale is established by resting the dynamometer on one knob and placing weights on the other knob; the position of the index for each weight is scratched on the plate. The knobs may be placed at any desired point along the rods. As they are placed nearer to the block, the apparatus becomes less sensitive and the movement less; as they are moved toward the ends of the rods, the apparatus becomes more sensitive. In the present investigation they are so placed that the maximum force usually exerted makes the index pass over the entire scale.

[1] SCRIPTURE, New Psychology, ch. III, London, 1897.

The instrument is graduated for one position of the knobs and must then be left unaltered. These dynamometers are so readily made that it is preferable to have a separate one for any special problem rather than to alter the knobs and change the scale.

The dynamometer is used by holding it up between the ends of the thumb and one of the fingers. The other fingers are kept away from the one used. The metal block may be allowed to rest lightly on the palm of the hand.[1]

The dynamograph. The dynamometer is turned into a dynamograph by means of a piston recorder or a recording capsule. The particular one used in the following experiments was made by fastening one of the extra glass cylinders of a HÜRTHLE piston recorder to one of the steel rods while the piston was fastened to the other rod. Pressure on the knobs caused the piston to descend in the cylinder and the air to pass through the rubber tube to the recorder. The cylinder is adjustable to any point on the rods; this regulates the amount of movement in the piston.

The recording point is rested against any smoked surface in the usual way. It repeats the movement of the piston on the dynamometer and consequently indicates the pressure exerted. To graduate the record the dynamometer is placed in a vise or a clamp and is subjected to pressure so that its index reads 1, 2, 3, etc. on the scale; the position of the recording point at each of these readings is marked on the smoked surface.

SCALE FOR THE THUMB AND FINGER.

Two points are to be determined for our scale of voluntary effort: 1, the relation of its units to the weight units; 2, its regularity.

The subject of the experiment takes the dynamometer in his hand. At the command "One" he exerts a light pressure; at the command "Two" a pressure intended to be twice as great; at "Three" three times as great; etc. At each pressure the recording point marks its excursion on the smoked surface; between records the surface is moved so as to keep the marks separate. The experiment is repeated several times. Then the dynamometer is placed in the vise, which is screwed up until the index indicates a pressure of 1^{kg}; a turn of the drum inscribes the line for 1^{kg} on all records. This is repeated for 2^{kg}, etc. The records are then read in tenths of a kilogram.

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[1] This dynamometer and the dynamograph are pictured in SCRIPTURE, New Psychology, Figures 4 and 24, London, 1897.

The following is a specimen record; the figures in the top line give the relative intensities of the efforts as intended, while the actual results of five experiments are recorded below them. The unit is the kilogram.

1	2	3	4
0.5	1.0	1.7	3.3
0.4	1.0	1.6	2.8
0.5	1.0	1.6	2.5
0.8	1.6	2.5	3.7
0.9	2.1	3.2	4.1

The question arises concerning the proper method for computing the results. If the values in each column represent identical processes, they should be added directly. This is the method which I have followed in reporting the results in the New Psychology (p. 218). Further consideration leads me to modify the procedure. The values for effort 1 are not intended to be one particular effort, but any convenient light effort to start with. Likewise the values for effort 2 are not attempts at a certain definite effort, but are attempts to double effort 1, which may be different for different experiments, etc. The proper procedure seems, therefore, to lie in measuring efforts 2, 3 and 4 by effort 1, as a unit; this is done by dividing all four records by the record for effort 1 in each experiment separately. The specimen record then takes on the following form:

1	2	3	4
1	2.0	3.4	6.6
1	2.5	4.0	7.0
1	2.0	3.2	5.0
1	2.0	3.1	4.6
1	2.3	3.6	4.6

The averages and mean variations are then computed in the usual way. The results for several observers are given in the following table:

TABLE I.

Subject.	1	d_1	2	d_2	3	d_3	4	d_4	n
I	1	29%	2.1	9%	3.5	7%	5.6	2%	5
II	1	14%	2.0	11%	3.3	21%	7.1	19%	5
III	1	25%	3.0	27%	5.7	33%	11.4	14%	4
IV	1	22%	2.2	14%	3.1	29%	4.6	24%	4

d_1, d_2, d_3, d_4, mean variations. n, number of experiments.

Scale for forearm and hand.

These experiments were made with a dynamometer constructed by Dr. Seashore (p. 60, above). A light wooden rod was hinged at one end to an upright; a coiled spring supported the rod in a horizonal position. Pressure on the rod at a given point deflected it downward; this point was chosen very near to the axis in order to make the movement a minimum. An index at the movable end of the rod passed over an arc graduated in grams.

The subject was seated with the hand and arm extended horizontally. At the signals he executed downward pressures intended to be in the relations of 1, 2 and 4. The results are given in the following table. The experiments were all made by me on Dr. Seashore on the same day in successive groups.

Table II.

	1	d_1	2	d_2	4	d_4	n
	1	12%	2.2	9%	3.8	11%	10
	1	17%	2.3	24%	3.9	23%	10
	1	23%	2.4	13%	4.2	17%	10
	1	26%	2.3	20%	4.1	28%	10
	1	19%	2.1	15%	3.3	14%	10
Mean	1	19%	2.3	16%	3.9	21%	50

d_1, d_2, d_4, mean variations. | n, number of experiments.

Conclusions concerning the scales.

The mental scale of exertion is a fairly definite affair. It varies considerably in different individuals, but is fairly constant for the same individual on a given occasion.

The question of how these scales are established by past experience is not touched upon; the problem for the experiments related to the actually existing scale.

Repeated voluntary efforts.

(Henry E. McDermott.)

The purpose of these experiments was to measure, not the fatigue of maximum pressure, but the fatigue resulting from many repetitions of a moderate pressure, thus showing the fatigue of the finger muscles under control of the will involving concentrated attention.

The instrument used was the dynamometer described above (p. 69).

The first person experimented on, A. G., was a High School student. I allowed him to give a desired pressure and then told him to relax the grip, and with the eyes closed to give the same pressure as before. His results expressed in dekagrams were as follows: 78, 75, 80, 76, 85, 85, 85, 85, 83, 80, 84, 85, 90, 85, 85, 86, 82, 85, 80, 83, 83, 83, 84, 84, 85, 86, 90, 89, 90, 90, 90.

At 90 the pointer touched the extreme of the scale and continued to do so for several seconds. In this set we see a tendency to gradually increase the grip as it is repeated; this is exactly the opposite of what was expected. The results fluctuate for a time and then for a few seconds become regular. On the average, however, they slowly, yet constantly, increase in strength.

The second person experimented on was also a High School student, F. C. His results were 55, 70, 65, 70, 70, 75, 74, 76, 80, 80, 80, 82, 74, 80, 83, 82, 86, 85, 85, 85, 85, 83, 84, 82, 81, 78, 82, 85, 86, 83, 80, 82, 82, 83, 83, 82, 83, 85, 85, 81, 76, 77, 79, 80, 80, 73, 74, 75, 80, 78, 80, 80, 85, 90, 90, 88, 90, 88, 90, 90, 90, 90, 90, 90, 90, 90, 88, 89, 90, 90, 85, 89, 89, 88, 88, 90, 90, 90. In this set we see nothing very different from the first, except that there was greater regularity at the start and that the difference between beginning and end was greater because the starting point was lower.

The third person experimented on was also a High School student, N. B. His results were, 30, 34, 32, 33, 32, 29, 32, 29, 30, 30, 32, 30, 34, 33, 31, 29, 29, 29, 29, 31, 31, 32, 29, 33, 35, 32, 35, 40, 45, 35, 39, 38, 40, 38, 39, 41, 40, 36, 32, 40, 40, 35, 36, 34, 30, 38, 35, 35, 35, 35, 36, 34, 36, 40, 40, 35, 40, 40, 42, 40, 39, 40, 50, 50, 48, 45, 50, 50, 48, 44, 45, 44, 40, 40, 43, 44, 45, 44, 41, 40. In this set we have a remarkable constancy of exertion. However, the tendency to increase is noticeable, as almost all the later results are greater than earlier ones. The greatest difference is 20, in contrast with 26 of A. G. and 35 of F. C.; both of which would have been even greater had the scale been longer.

The fourth subject was a college student, J. R. N. His results were 47, 55, 50, 51, 55, 60, 62, 56, 60, 64, 66, 65, 71, 65, 72, 68, 68, 70, 66, 65, 68, 75, 76. In this set we have an almost constant increase up to a maximum of 29, which is large considering the shortness of the experiment.

The fifth subject was D. J. R., a carpenter. His results were 55, 53, 54, 65, 66, 68, 65, 66, 77, 75, 73, 72, 78, 78, 80, 82, 85, 85, 87, 85, 82, 83, 80, 82, 81, 85, 87, 90, 85, 87, 90; 90, 90, 90. At 90 the

pointer touched the extreme of the scale. Here we see the same steady increase up to the limit of the instrument.

The sixth subject was a young lady, Miss F. Her results were 45, 47, 36, 42, 45, 50, 50, 44, 43, 51, 55, 56, 55, 46, 40, 47, 52, 54, 60, 54, 56, 40, 49, 53, 47, 49, 48, 54, 45, 56, 58, 57, 60, 60, 54, 61, 65, 70, 74, 74, 74, 72, 59, 52, 58, 66, 55, 70, 71, 72, 68, 70, 70, 64, 68, 65. Here we have the same general result, except that it is more irregular than the previous ones.

The preliminary experiments seemed to point to something definite, so for seven successive days at about 6.30 P. M. I made sixty experiments on D. J. R. to determine his average progressive error and mean variation. He is a carpenter, 24 years of age, 5 feet 11 inches (180.6^{cm}) in height and weighs 175 lbs. (79450^{g}). He uses his hands in hammering, sawing, planing and so on; he is therefore not easily fatigued, and at the same time his judgments by the "muscle sense" should be accurate because of this training.

In the following table the lines across the page give the averages for the experiments of the seven days. The first line gives the serial number of the contraction; the second line gives the difference—"progressive error"—between each contraction and the initial contraction of 20 dekagrams. The third line gives the mean variations.

Serial number	1	2	3	4	5	6	7	8	9	10	11	12
Progressive error	0	0	0.3	0.1	1.3	1.9	2.3	1.9	1.9	2.6	2.0	3.9
Mean variation	0	1.7	2.3	2.7	2.9	2.8	3.8	4.4	5.2	4.8	3.7	4.2
	13	14	15	16	17	18	19	20	21	22	23	24
	2.7	3.6	3.6	4.3	3.7	3.0	4.1	3.7	4.0	3.1	3.9	4.3
	3.6	4.2	4.7	7.0	5.4	4.9	5.0	4.4	5.4	6.2	5.2	5.0
	25	26	27	28	29	30	31	32	33	34	35	36
	5.6	5.4	5.4	4.6	5.1	5.9	4.7	5.3	5.3	5.4	5.6	6.3
	7.4	8.3	8.0	7.7	8.2	7.2	8.2	7.8	9.0	7.2	8.0	8.8
	37	38	39	40	41	42	43	44	45	46	47	48
	6.6	4.9	5.6	5.7	5.6	6.4	6.0	4.6	5.0	4.4	4.4	4.6
	9.7	7.8	7.7	7.6	8.3	7.7	7.1	6.7	7.1	6.9	7.1	6.1
	49	50	51	52	53	54	55	56	57	58	59	60
	3.4	4.0	4.3	6.3	6.3	4.7	3.7	3.7	4.3	7.0	7.0	6.4
	6.7	7.0	5.7	6.7	6.0	6.2	5.7	7.1	8.9	8.3	8.1	7.7

These results are exhibited in Figure 5.

Using "fatigue" to mean a decrease in functional activity as defined on p. 14, we can consider both the progressive error and the mean variation in the preceding experiments to be phenomena of fatigue. Although

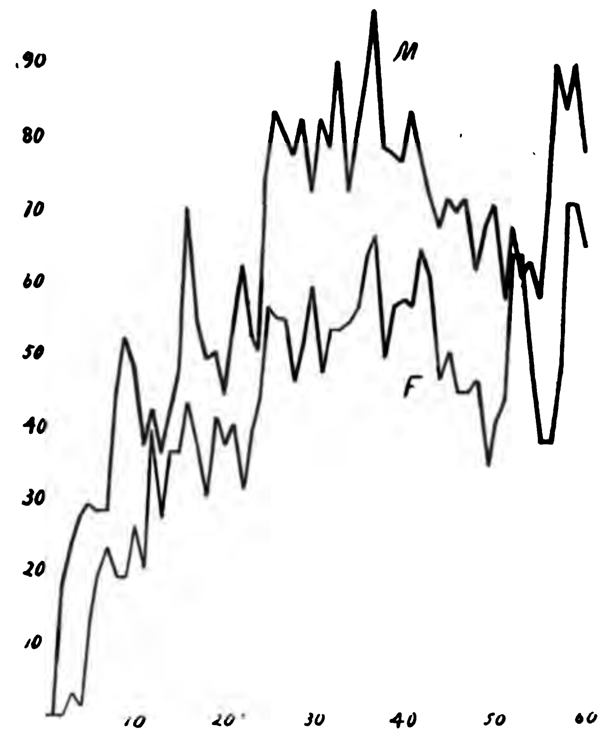

Fig. 5.

Horizontal axis, serial number of experiment.
Vertical axis, number of grams.

F, course of progressive error.
M, course of mean variation.

the actual force exerted increased as the efforts were repeated, yet, since they were intended to be equal to the first one (or to the preceding ones), they became steadily less accurate. This increasing inaccuracy of judgment is properly a phenomenon of fatigue. Likewise the increasing uncertainty, as measured by the mean variation, is a constantly recurring phenomenon of fatigue.

NEW APPARATUS AND METHODS.

BY

E. W. SCRIPTURE.

Those pieces of apparatus which have been developed for purposes of research have been in general described in connection with the investigations for which they were first used. There remain two classes of apparatus for special description. The first class is that of general utility for all purposes; the second that for demonstration purposes. Both classes are largely the effects of the increased numbers of students, whereby it becomes necessary to provide labor-saving utility-pieces and practicable means for demonstrations on a large scale.

LAMP BATTERIES.

Long experience with galvanic and storage batteries of many sorts made it evident that some method must be devised by which the city current could be made available for all the battery work of the laboratory. As the Yale laboratory is supplied with the 110-volt direct current, the problem was reduced to that of finding a method of readily transforming it at any point in the building to a current of lower voltage.

A motor-dynamo, or motor-transformer, was considered. This machine is attached to the supply wires at any point; two wires leading from it furnish the current at the particular voltage for which the machine was built. It is, however, quite costly and also inconveniently heavy. A laboratory of any size can hardly do with an equipment of less than ten batteries; such a set of motor-dynamos would be quite beyond the reach of most institutions. A larger motor-dynamo might be used to distribute a low-voltage current throughout the laboratory by a special set of wires. This method is open to many objections; it need not be considered when the laboratory receives the 110-volt direct current, as the lamp batteries offer a better solution. When, however, an alternating current or one of very high voltage is received, the proper method would presumably be to transform it to a direct one of 25 volts and send it through the laboratory to be used from sockets by means of the appropriate lamp batteries.

It was suggested by Prof. A. WRIGHT, of the Physical Laboratory, that a shunt arrangement might be made by means of lamps in such a manner as to yield a current of the desired amount and tension. This was tried,

but owing to the lack of lamps suitable for the shunt, a coil of wire was used.[1] The arrangement was fairly successful, but was finally abandoned. A study of catalogues of incandescent lamps showed that the original idea was a possible one; thus the lamp battery was finally developed.

The principle of the lamp battery may be explained by describing the method of construction. A convenient form of battery is made as follows: A base-board 10 x 6 inches (say 25 x 15 centimeters) is sawed from a board 1 inch (2½ centimeters) thick. It is convenient to keep a supply of such bases varnished and ready for use, as an extra battery may be

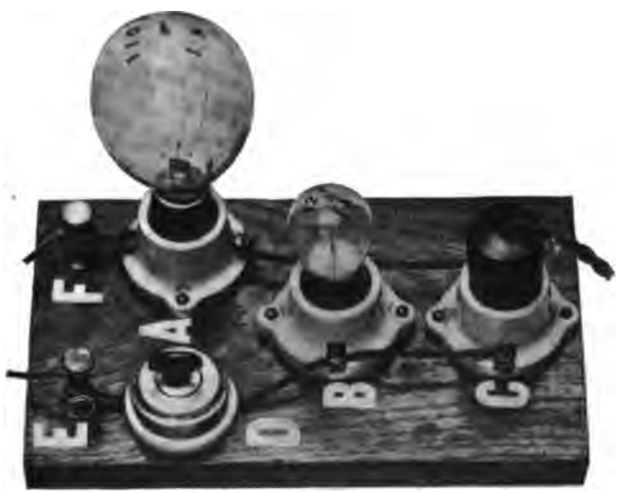

FIG. 6.

required at any time. Three lamp sockets (so-called "wall receptacles") a snap switch and two binding-posts are then screwed to the board in the positions indicated in Figure 6. Socket *A* is for lamps with the same base as that used on the regular supply circuit for lighting the building; sockets *B* and *C* are for lamps with a different base. For example, I use the T-H base for *A* and the Edison base for *B* and *C*. Thus it is impossible to place a lamp in the socket not intended for it. The battery is wired with the usual silk-covered lamp cord, the ends being neatly tied with thread. The method of wiring is sufficiently indicated in the figure. A supply of plug wires is prepared. What I call a "plug wire," for lack of a better term, is made by connecting the ends of an ordinary

[1] SCRIPTURE, *Some new apparatus*, Stud. Yale Psych. Lab., 1895 III 109.

lamp cord (6 feet, or 2 meters) to a socket plug; the other ends are scraped, bound with thread and left free.

To use the battery a plug wire is inserted in any lamp socket on the supply circuit. The free ends are brought to the binding-posts *E* and *F*. A 110-volt lamp of the required ampèrage is placed in *A*. Thus for an electric fork a 1-ampère lamp will be used, for a spark coil a 4-ampère lamp, etc. A low voltage lamp of the same or greater ampèrage than the one in *A* is inserted in *B*. Thus for the electric fork the lamp must carry at least 1 ampère in order to correspond with the lamp in *A*; it may conveniently be of 10 volts. For the spark coil a lamp of 8 volts 4 ampères would be suitable.

A plug wire is now placed in *C*, and the switch *D* is snapped to turn the current on. At the ends of the wires from the plug in *C* a current can now be drawn whose maximum intensity is practically the same as that in the lamp *A* and whose tension is practically the same as that at the poles of the lamp *B*. In the case of the electric fork it would be a current of 10 volts 1 ampère; for the spark coil it would be 8 volts 4 ampères.

The lamp battery behaves like any other battery. Increased resistance in the external current decreases the intensity of the current delivered, etc. For circuits of great resistance a lamp of higher voltage may be used in *B*. For larger currents than 4 ampères the sockets at *A* and *B* are doubled, as it is not advisable to use the ordinary socket for a current of more than 4 ampères on a 110-volt circuit.

The character of the lamp batteries can be seen from the following tables.

TABLE I.

Lamps used in the batteries.

LARGE LAMPS.

Mark on Lamp.	Trade Name.		
A	110 volts	100 c. p.	4 ampères.
B	110 volts	100 c. p.	3½ ampères.
C	110 volts	64 c. p.	
D	110 volts	32 c. p.	
E	110 volts	16 c. p.	
F	110 volts	8 c. p.	

SMALL LAMPS.

Mark on Lamp.	Trade Name.	
m	8 volts	4 ampères.
n	8 volts	4 ampères.
o	8 volts	4 ampères.
p	12 volts	3 ampères.
q	12 volts	2 ampères.
r	12 volts	1 ampère.
s	12 volts	0.7 ampère.
t	10 volts	1 ampère.
u	6 volts	1 ampère.
v	20 volts	16 c. p.

TABLE II.

Results of various combinations of lamps.

Lamps used.	Am	An	Ao	Bm	Bn	Bo	Bv	Cm	Cn	Co	Cp	Cq	Cv
Potential in volts.	9	5	7	7	5	6	37	4	3	4	7	10	25
Max. cur. in amp.	4.0	4.0	4.0	3.5	3.5	3.5	3.5	1.9	1.9	1.9	1.9	1.9	1.9

Lamps used.	Dp	Dq	Dr	Dt	Du	Dv	Eq	Er	Es	Et	Eu	Ev	Fs	Fv
Potential in volts.	4	5	11	10	5	15	3	5	7	4	2	8	4	6
Max. cur. in amp.	1.0	1.0	1.0	1.0	1.0	1.0	0.5	0.5	0.5	0.5	0.5	0.5	0.3	0.3

As it is sometimes desirable to distinguish the poles of the battery, the left hand wire has a red covering, while the right hand wire has a green one, and all the sockets are so placed that the central contact is connected with the red wire. For the plug wires I use a twisted red and green cord, with the red cord connected to the central contact. All sockets on the supply wires have the central contact connected with the positive wire. Thus all central contacts and all red wires are positive.

FIG. 7.

Some of the lamp batteries are wired in a slightly different manner. The purpose of the new wiring is to render it possible to use two circuits in parallel. For example, to run a DEPREZ marker with a 100 v. d. electric fork, having a low resistance magnet, the fork should not be placed in series with the marker as the current running through the fork would be greatly reduced by the high resistance of the marker. The proper way is to run the wires from the battery to the marker and then

place the fork as a shunt around the marker. Every time the fork makes contact the current pulls the prongs but is shunted from the marker because the fork offers so much less resistance, whereas at every break of contact the current is forced through the marker. Thus a large current may be used for the fork and a small one for the marker. This method of connection may be arranged with the lamp battery just as with other batteries, but a further improvement may be made. A fourth socket, *G*, is placed in series with the socket for the small lamp, as shown in Figure 7. The lamps are arranged as before; the plug wire from *C* is connected with the fork and another plug wire from *G* with the marker. The battery runs the fork as usual. Every contact of the fork shunts the current from *B* and *G*; every break of the fork, however, forces the current at a tension of 110 volts through the marker. The advantage of this arrangement is frequently very great. This form of battery may be conveniently termed the "extra circuit battery." By closing *G* with a metal plug the battery may be changed to the same system as Figure 6.

To save time and space I mount several lamp batteries on a single board and fasten it on the wall where it is likely to be used. Figure 8 shows such a battery board placed over the drum-table in the time-room. The switch at the top turns off the current completely; the socket in the middle gives direct access to the 110-volt supply, e. g., for running a motor. From the fuse block at the bottom the wires divide to the four batteries, each battery having its separate switch. Two of the batteries are wired by the method shown in Figure 6. The first and third, counting from the left, are wired after Figure 7; the plugs for closing the extra sockets hang at the sides when not in use. The extra switch introduced in the latter batteries enables further changes, but the arrangement is too complicated for ordinary use. The cost of a lamp battery is much less than that of a corresponding galvanic battery; the expense of running it on the city circuit is trifling; the only renewals are those of the lamp when it is burned out; a new battery can be made in ten minutes; the saving of time formerly required for setting up or replenishing batteries is worthy of consideration.

Fig. 8.

Multiple key.

The multiple key described in the first number of these Studies (pp.

10, 97) has undergone further improvement. In addition to changes in execution such as greater lightness of the parts and fineness of workmanship, the following important alterations have been made. 1. Another break contact has been added to the rear end of the lower lever; thus two circuits can be broken simultaneously by pressure on the key, with or without closing one or two other circuits at the same movement. This double break is not only very useful on many occasions, but is sometimes indispensable. 2. The point which dips into the mercury cup has been made adjustable. 3. The main contacts are independent; this re-

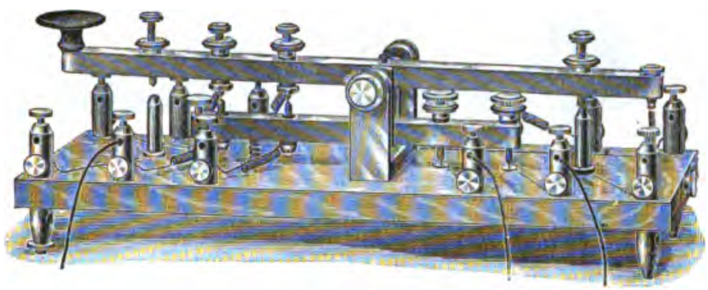

FIG. 19.

quires twelve binding posts. 4. All connections are made on top so as to be directly visible. The system of connection can be seen in Figure 19. The wire from the further post on the front of the key leads to the mercury cup which is hidden by the third post from the left in front in the figure.

In this connection it may be well to mention that, when this key (or any other key) is used with the spark coil, the condenser should be connected around the place where the circuit is broken. The spark coil should have a separable condenser. When a coil is to be bought, it may be ordered to be built in three separate pieces; the primary coil, the secondary coil and the condenser. The condenser can then be used anywhere. The independent primary is useful for teaching the construction and use of the spark coil. The magnetic interrupter is not needed; by omitting it separable the form of the spark coil becomes cheaper than the usual one.

ADJUSTABLE SUPPORT FOR RECORDING INSTRUMENTS.

For the convenient adjustment of forks and markers so that they may write properly on the revolving drum I have devised the support shown in Figure 10.

The horizontal rod is fastened in any desired position to the upright rod of the drum-carriage by means of the clamp *M* with the screws *A* and *B*. The horizontal rod *T* runs through the hollow rod *P*. At its end is a screw-thread on which the nut *F* is placed. The nut *F* is so adjusted that the hollow rod *P* plays freely but without shake around the horizontal rod. The jamb-screw *E* locks *F* in position. The upper arm *Q*

FIG. 10.

with the screw *N* is fast to the horizontal rod, and the arm *O* to the hollow rod *P*. The arm *O* is held against the screw *N* by a spring.

If a fork is to be used, a hole is bored in the wooden base; it is then placed on *R* and screwed tightly down by means of *S*. The fork is then brought into rough adjustment and fastened by *I* on the rod *P*. If a marker is used, it is placed directly on *P*.

The finer adjustment is done by the screw *N* which slowly moves the arm *O* and lowers or raises the fork. Pressure of the finger on *O* lifts the recording point at once from the paper. A view of a fork applied to the drum in this way is to be seen in my New Psychology, Figure 6.

The clamp *M* may be of brass or hard rubber, the latter being necessary when the spark method is used in combination with a marker. (See p. 117 below.)

SYSTEM OF PROJECTION.

Although the requirements of psychology in the matter of projection are in some respects similar to those of other sciences, there are certain important peculiarities that must be borne in mind in providing lanterns, screens, etc. For single, plain slides, the equipment may be the usual one

with an ordinary lantern, and for projection of apparatus the open-work lantern may be used as in physics. Yet these methods leave untouched the subjects of color and binocular vision, which are specifically psychological and which require lantern-work when presented to large classes.

In the following account I shall describe the system which I have developed at Yale.

Triple lantern.—The projection in colors requires a triple lantern of special construction. For stereoscopic projection two of the parts and for plain projection one or two or three parts are used.

The triple lantern which we possess is shown in Figure 11. It is arranged for lime-light, as the color work cannot be done with electric or acetylene light. The three jets are packed closely into one lantern-body. The three condensers are as close together as possible. Three lenses exactly alike are mounted on the front board. The jets have all adjust-

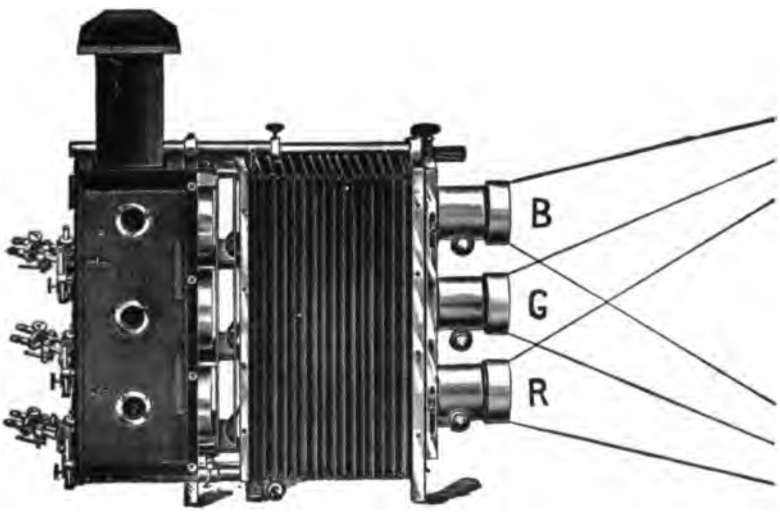

Fig. 11.

ments for regulating the gas, manipulating the lime, etc. Limes turned in the lathe are used in order not to disturb the focus as they are rotated in the lantern. Regulators are placed on the cylinders.

To mount the lantern a shelf of 1½ inch (4 centimeters) pine is built out from the wall and rigidly supported; every precaution is taken to prevent warping. A plate of thick wood or of iron is then prepared just large enough to form a base for the lantern. The lantern is screwed

to this plate. The proper position for the lantern is found; the plate is made perfectly level and is then either screwed to the shelf or is so marked that it can always be brought readily to its proper position. The screen is put in place and the three lenses are brought into perfect registration.

Apparatus lantern.—A regular open-work lantern may be used, but I find it preferable to build one in the following way. A pine board four feet (1 1/4 meter) long and nine inches (23 centimeters) wide serves as the base. Across it at about one foot (30 centimeters) from the end there is an upright board one foot (30 centimeters) high supported on a narrow base. An opening is made toward the top of this board to admit the condenser-lens. The projecting lens is held by another upright board supported on a base and not attached to the horizontal board. The light, which may be one of the jets from the triple lantern, is held by a rod on a free metal base which is placed at the desired point behind the condenser. One of the condensers of the triple lantern may be readily removed and used in this lantern. The projecting lens may be a very simple one. The large open base and the free adjustment of all the parts make the arrangement of apparatus very easy.

Screens.—Where the room is very high, it is desirable to have the pictures back of the lecture room table. In this case only one screen would be needed; a silvered screen of the appropriate size. When the room is not so high, the silvered screen is placed on rollers just above the front edge of the experiment table; for projections where part of the apparatus is used on the table and part is projected by the lantern another screen is used back of the table. For this latter screen I use muslin sheeting long enough to reach entirely across the room. It is supported by rings on a tightly stretched cord and is readily drawn across or pulled back by other cords. The use of so wide a screen enables me to project two views simultaneously side by side.

The silvered screen is required for stereoscopic work by polarized light; moreover, it is far more brilliant than any white screen.

Single views.—For single views two of the lanterns of the triplet may be used for dissolving. A lantern view is more effective than a chart for various reasons: it is not exposed until it is wanted; it is brilliantly lighted while all else is dark; it must be strictly attended to because it will speedily disappear. Views of apparatus should also be used wherever possible. The actual piece of apparatus is so small that the details are invisible to distant students; a lantern view of the whole or of some essential part greatly aids the understanding. I also find that even the most striking and brilliant instruments rarely command the same attention as a lantern view.

Apparatus views.—For apparatus projection several methods may be followed. Whenever possible, I place as much of the apparatus as I can in full view on the experiment table; the registering part is then placed in the apparatus lantern and projected to one side of the screen behind the table, while the recording point is prolonged so as to write on a smoked glass plate in one part of the triple lantern which projects the record beside the view of the recording apparatus. When this cannot be done, one or two lanterns may be used as desired.

Stereoscopic views.—For stereoscopic projection two methods may be used.

By the color method sheets of red and green gelatine (for lime light) or glass (for electric light) are placed before the condensers of two of the lanterns. Small squares of red and green glass are held before the left and right eyes respectively. The gelatines and the glasses require careful selection. An ordinary white screen may be used.

With the other method two polarizers are placed in front of two of the lanterns with the axes of polarization at right angles.[1] The stereoscopic view-slides are projected simultaneously to the same place. Each student receives an eye-glass consisting of two analyzers with axes of polarization at right angles to each other. The views are thus received by the eyes separately and the result is an object apparently in full solidity. The whole subject of binocular vision lends itself to treatment by this method. Series of stereoscopic slides have been specially prepared for this purpose.

Color views.—For color projection three colored films, red, green, and blue, are placed in the triple lantern. I have devised a slide which shows on the screen the elementary colors singly with their combinations in pairs and in triple. Shades are shown by slowly turning the light down. The various hues and the laws of combination are illustrated by varying the intensities of the jets. The properties of the color triangle and the color pyramid are thus illustrated. When the laws of color are thoroughly impressed by this method, slides of concrete objects are used for study. Thus a group of flowers, Figure 12, affords an illustration of the automatic solution of color equations. The matter is quite complex, but a few illustrations will serve to show how I approach the subject. Every color is the sum of three elements, or $i = x + y + z$. Assume for these elements the three colors red, green, and blue, and let x indicate the number of units of red, y of green, and z of blue. For white we have equal parts of the three elements, or $x = y = z$. The middle stalk of flowers in each slide allows the light to pass fully through it, as can be

[1] A view of these polarizers adjusted to a biunial lantern is shown in SCRIPTURE, New Psychology, Fig. 113, London, 1897.

seen when the views are shown singly. When all three are projected together, it will receive equal illumination from each, and will consequently be white. The stalk on the left in the *R* slide allows the red light to pass, but keeps it entirely back on the *G* and *B* slides, consequently we have $y = 0$ and $z = 0$ and $i = x$; that is, the stalk appears red in the final picture. In this manner the greens, blues, yellows, purples, etc., with their various hues, tints, and shades may be worked out.

FIG. 12.

"The phenomena of color blindness can also be represented with the tricolor lantern. The usual theory of color blindness, according to which the defect arose by the failure of one of the three fundamental colors, can be illustrated by covering up one of the lenses. For red blindness the red lens is covered, and the resulting picture appears in combinations of green and blue; for green blindness the green lens is covered and for the hypothetical blue blindness the blue one is covered. To illustrate the newer theory, the blue slide is left unchanged, but two slides are made for red and two for green. For the dichromats of the first class—the red-blue persons—the two slides taken through the red ray filter are placed in the red and green lanterns. Thus, in the case of the gladiolus, the slide *R* is thrown on the screen in red light from one lantern, and also in green light from the second lantern, while the *B* slide is thrown in blue as usual. The *G* slide is not used. The result is a picture in combinations of yellow and blue. For the other dichromats—the green-blue persons—the *G* slide is thrown in red light, and again in green light, while the blue remains the same. The *R* slide is not used. The result is also a picture in combinations of yellow and blue, but each particular combination differs from that in the previous

cases. To illustrate monochromasy one lantern alone is used, the color being left to an arbitrary choice."

The method also furnishes a remarkable analogy to the decomposition of the colors by the eye into three fundamentals and their mental recomposition into sensations of color. The tricolor views are taken by a camera used three times in succession with a differently colored screen each time. The red rays impress one of the plates, the green rays the second and the blue rays the third. The three negatives differ in their shading. Three positives are made which differ likewise, as in Figure 12. The three positives produce views appropriately shaded when projected on the screen by the colored lights. The result is a recomposition in natural colors. The approximation to the original colors is close if the slides are properly made and manipulated.

SLIDE MAKING.

The constant use of lantern views for instruction renders it necessary to provide an equipment for the making of slides at a minimum expense of time. The system which I shall describe was developed expressly for the rapid production of slides for class-room work and of photographs for illustrating books.

The photographic section of the Yale laboratory is located directly under the roof and receives light from a skylight. For work at night the objects to be photographed may be illuminated by burning pieces of magnesium tape. This luminant is used instead of electric light or lime light, because the number of photographs taken at night has been too small to render a regular installation profitable.

For photographing apparatus a table on rollers is provided. White, gray and black backgrounds and table covers are provided to fit on the top of the table. A cover is placed on the table, backgrounds and side-pieces are arranged and the apparatus is placed in the compartment thus formed. The table is then moved until the appropriate lighting is obtained. The choice of white, gray or black for the walls of the compartment is not always an easy matter unless one has had considerable experience with the reflecting qualities of the metals; white is, however, generally used. The strong reflections on polished metal produce sharp streaks of black and white. I find it generally preferable to photograph nickeled or burnished pieces after they have been in use and have slightly lost their polish.

For reproducing drawings, photographs and pictures in books the camera is mounted on a heavy block moving on rails; it may be raised, tipped sidewise or rotated at will. Loose pictures are tacked to a board

at the end of the rails; books are placed on blocks and held open against the board by a rubber band. The adjustment of the camera can be rapidly performed.

When the blocks used to illustrate books are at hand, slides can be printed directly from them.[1] It is best to have the work done by the glass-printer in a clock factory. The metal portion of the cut is mounted on a board of a thickness suited to the particular frame used in the printing. It is inked with a fine ink (e. g., a $2 cut or extra job ink), tempered to the proper consistency with Calcutta boiled oil and Japan drier. The precise degree of temper depends on temperature, humidity, and other conditions. The inking is done by a simple hand roller of the kind used in ordinary printing. The block lies face upward on the table and the piece of plain glass is placed at the appropriate distance on a level with it. A composition roller of glue and molasses, made a trifle harder than the regular printer's roller, is then run forward on two guides. As it passes over the block it takes the impression. On reaching the glass, after one complete revolution, it transfers the ink impression directly to it. I do not think it possible to run this roller evenly enough without the guides; at any rate, it would not pay to waste time in trying it.

The result is a print on the glass just as if on paper. Curiously enough, the prints on the glass are superior to those on paper from the same block. The positives are then finished up as lantern slides in the usual way.

[1] SCRIPTURE, *A new method of making lantern slides*, Scientific American, 1895 LXXIII 123.

ELEMENTARY COURSE IN PSYCHOLOGICAL MEASUREMENTS.

BY

E. W. SCRIPTURE.

Owing to the newness of experimental psychology its methods of instruction are still matters which must be determined by trial. SANFORD[1] has developed a course of simple laboratory experiments, but otherwise the problems of systematized laboratory instruction remain unsolved. A very important problem is that of systematic courses in psychological measurements. Among such courses there must be an elementary one. As the results of my experience of five years in trying to develop such a course may be of use, I will illustrate the methods employed by describing some of the exercises.

The aim of this elementary course is similar to that of the elementary courses in chemistry and physics, namely: education and instruction of the general student. It is intended to be part of the regular college education; among elective courses it is specially chosen by students intending to study medicine or to teach. It is a noteworthy fact, however, that the subject matter attracts students who take no other laboratory courses of any kind.

The student makes his own text-book with the aid of: 1. sets of mimeographed instructions which are given out at each exercise; 2. illustrations in the form of prints from blocks, blue-prints, tracings, etc.; 3. references for applications and further reading to some psychological work. The following are copies of some of these mimeograph-sheets with explanatory remarks. The "Preliminary notes" are given out with the first exercise. The first few exercises are of moderate difficulty, but they occupy the inexperienced student for about two hours each. The later exercises are adapted to the increased skill of the student. Since the applications and the bearings of the exercises can be made evident only in a general course on experimental psychology, the laboratory course is taken only in connection with the lecture course. Some idea of the verbal instruction that is given to the pupils and of what they hear and see in the lecture course can be obtained by referring to my New Psychology by means of the topics in the index.

The following exercises are selections from a set of thirty now in use.

[1] SANFORD, Laboratory Course in Experimental Psychology, Boston, 1895.

PRELIMINARY NOTES.

A. Objects of the course.—Practical training in (1) observation, (2) manipulation, (3) computation, (4) deduction, (5) criticism. Elementary acquaintance with (*a*) methods of experimentation, (*b*) methods of measurement, (*c*) construction and use of apparatus, (*d*) special psychological methods. Thorough appreciation of the three fundamental properties of scientific work: (1) accuracy, (2) brevity, (3) neatness.

B. Arrangement of the class.—The class is divided into groups of two persons each. Any student who wishes to do so may select the other member of his own group. One group begins with Ex. I.; another with Ex. II., etc. At the next exercise the group that has had Ex. I. takes Ex. II.; the one that has had Ex. II. takes Ex. III.; etc. At each succeeding exercise a group takes the exercise that follows in numerical order.

C. Instructions to the student.—Look at the index on the bulletin board; opposite the number of the exercise for the day you will find the number of the room in which it has been set up. On the table bearing the number of your exercise you will find two sets of printed instructions, one for each person. Compare your set with another set marked in red "Corrected Copy," and make any changes that have been indicated in red ink. You will also find all the apparatus of the exercise called for under "Needed." It is set up ready for use. Begin by reading the first paragraph of the instructions and applying it to the apparatus. Take the following paragraphs singly.

After carefully studying the apparatus and its connections take it down and set it up again. In performing the experiments one person serves as experimenter, the other as subject. The places are then exchanged and the experiments are repeated. The subject is to know nothing about the results obtained on himself. The record must be made on the printed blanks.[1] When finished, these records are to be handed to the instructor. They will be marked, the mark being given to the experimenter (who has prepared the record).

The fundamental requirements for the records are accuracy and neatness. See that you understand all the "Points to be noted." If not, consult the instructor. Also see that you can answer the "Questions." It is intended in many cases that you shall get the answers directly from the instructor. The student will be held responsible on all these points. At the end of the exercise place all apparatus in the condition in which it was found. Do not leave until the instructor has inspected and approved your work. When the whole class has finished an exercise, the

[1] A specimen blank will be sent to any one who will ask for it.

final results for each student will be placed in a table and a copy of the table will be handed to the student, together with his record. This "summary" and the original records are to be kept by the student. The examination at the end of the term will include some practical work.

GENERAL INSTRUCTIONS FOR COMPUTATION.

If the method of measuring is sufficiently fine, the results in a set of measurements will differ from each other. The average of these is the most probable value of the quantity. Let the results of a set of n measurements on the same quantity under constant conditions be m_1, m_2, . . . , m_n. The average is

$$a = \frac{m_1 + m_2 + \ldots + m_n}{n}.$$

It is desirable to have some expression of the uncertainty of the result a, and this is given by the "probable error." The probable error is that error which is as likely as not to be exceeded, or, if r be the probable error of a, it is just as likely that the true value of the quantity lies between $a - r$ and $a + r$ as that it lies outside those limits. Thus, if the average be written $a \pm r$ the probable error r furnishes an index of the uncertainty of a; the smaller the value of r, the greater is the precision of the average a. In works on the theory of errors it is shown that the probable error r is given by the expression

$$r = 0.6745\sqrt{\frac{v_1^2 + v_2^2 + \ldots + v_n^2}{n(n-1)}},$$

in which v_1, v_2, . . . , v_n are the residuals found by subtracting the individual measures from the average, thus $v_1 = a - m_1$, $v_2 = a - m_2$, . . . , $v_n = a - m_n$.

As an example let there be ten measurements made with equal precision upon a single quantity giving the results $m_1 = 10$, $m_2 = 14$, etc. The computation is as follows:

m	v	v^2	
10	1.2	1.44	
14	2.8	7.84	
16	4.8	23.04	
8	3.2	10.24	$\sqrt{0.91} = 0.95$
14	2.8	7.84	
6	5.2	27.04	
12	0.8	0.64	$\frac{2}{3} \times 0.95 = 0.63$
10	1.2	1.44	
12	0.8	0.64	
10	1.2	1.44	$a = 11.2 \pm 0.6$
10)112		9)81.60	
$a = 11.2$		10)9.07	
		0.91	

Here the average is 11.2 and its probable error is 0.6; that is, it is as likely that the true value lies between 10.6 and 11.8 as that it is less than 10.6 or greater than 11.8. If the observations had been such as to have given $a = 11.2 \pm 0.9$ the average 11.2 would be much less precise than in the above case.[1]

TABLE OF SQUARES.

1	1	21	441	41	1681	61	3721	81	6561
2	4	22	484	42	1764	62	3844	82	6724
3	9	23	529	43	1849	63	3969	83	6889
4	16	24	576	44	1936	64	4096	84	7056
5	25	25	625	45	2025	65	4225	85	7225
6	36	26	676	46	2116	66	4356	86	7396
7	49	27	729	47	2209	67	4489	87	7569
8	64	28	784	48	2304	68	4624	88	7744
9	81	29	841	49	2401	69	4761	89	7921
10	100	30	900	50	2500	70	4900	90	8100
11	121	31	961	51	2601	71	5041	91	8281
12	144	32	1024	52	2704	72	5184	92	8464
13	169	33	1089	53	2809	73	5329	93	8649
14	196	34	1156	54	2916	74	5476	94	8836
15	225	35	1225	55	3025	75	5625	95	9025
16	256	36	1296	56	3136	76	5776	96	9216
17	289	37	1369	57	3249	77	5929	97	9409
18	324	38	1444	58	3364	78	6084	98	9604
19	361	39	1521	59	3461	79	6241	99	9801
20	400	40	1600	60	3600	80	6400	100	10000

EXERCISE I.—THRESHOLD OF TOUCH.

(Needed: touch-weights, cross-section paper, flexible ruler.)

Apparatus.

The set of touch-weights consists of small cork discs weighing from 2^{mg} upward; they are attached by fine threads to small handles.[2] The weights are marked on the handles.

Experiments.

The subject, with eyes closed, places his left hand, palm upward, on his knee. He is to tell when he feels himself touched. The experimenter gives the warning "Ready" and, about 2 to 5 sec. later, lowers the lightest disc gently till it touches a certain spot on the skin, e. g.,

[1] I am under great obligation to Professor Mansfield Merriman (author of "A Text-book on the Method of Least Squares"), of Lehigh University, for assistance in presenting the methods of computation.

[2] See Fig. 57 of SCRIPTURE, Thinking, Feeling, Doing.

the tip of the index finger; the disc is allowed to rest on the skin for about 1". The experiment is then repeated with the next heavier disc, and then with the other discs in succession. A check is made in the appropriate column of the record blank for each disc as it is felt. The whole experiment is repeated 10 times.

After the whole set of experiments has been made the number of the first disc felt in each experiment is recorded in the column headed m_1, and the number of the disc beyond which all were felt in the column headed m_2. The average is calculated for each set. The former of the two averages may be called the lower threshold, the other the upper one.

Specimen record.

Title of investigation, Threshold of touch.
Experimenter, T. C. McGraw.
Experimented on, W. K. Chisholm.

Apparatus, Touch-weights.
Date, October 5, 1896.
Unit of measurement, milligram.

Weight	2	4	6	8	10	12	14	16	18	20	22	24	26	28	30
Exper. No. 1	.	.	.	.	+	+	+	+	+	+	+	+	+	+	+
" " 2	.	.	.	.	.	.	+	+	+	+	+	+	+	+	+
" " 3	.	.	.	.	.	.	.	+	+	+	+	+	+	+	+
" " 4	.	.	.	+	.	+	+	.	+	+	+	+	+	+	+
" " 5	.	.	.	.	.	.	+	+	+	+	+	+	+	+	+
" " 6	.	.	+	+	+	+	+	+	+	+	+	+	+	+	+
" " 7	.	.	.	.	.	+	+	+	.	+	.	+	+	+	+
" " 8	.	.	.	.	+	.	+	+	+	+	+	+	+	+	+
" " 9	.	.	.	.	.	+	+	+	+	+	+	+	+	+	+
" " 10	.	.	.	.	+	+	.	+	+	.	+	+	+	+	+
Times felt	0	0	1	2	4	6	8	9	9	9	9	10	10	10	10

m_1	v_1	v_1^2	m_2	v_2	v_2^2
10	1.2	1.44	10	5.0	25.00
14	2.8	7.84	14	1.0	1.
16	4.8	23.04	16	1.0	1.
8	3.2	10.24	18	3.0	9.
14	2.8	7.84	14	1.0	1.
6	5.2	27.04	6	9.0	81.
12	0.8	0.64	24	9.0	81.
10	1.2	1.44	14	1.0	1.
12	0.8	0.64	12	3.0	9.
10	1.2	1.44	22	7.0	49.
11.2		9)81.60	15.0		9)252.00
		10)9.07			10)28.00
		0.91			2.80

$\sqrt{0.91} = 0.95$ $\qquad$ $\sqrt{2.80} = 1.67$

$\frac{2}{3} \times 0.95 = 0.63$ $\qquad$ $\frac{2}{3} \times 1.67 = 1.11$

$a_1 = 11.2 \pm 0.6$ $\qquad$ $a_2 = 15.0 \pm 1.1$

Theoretical considerations.

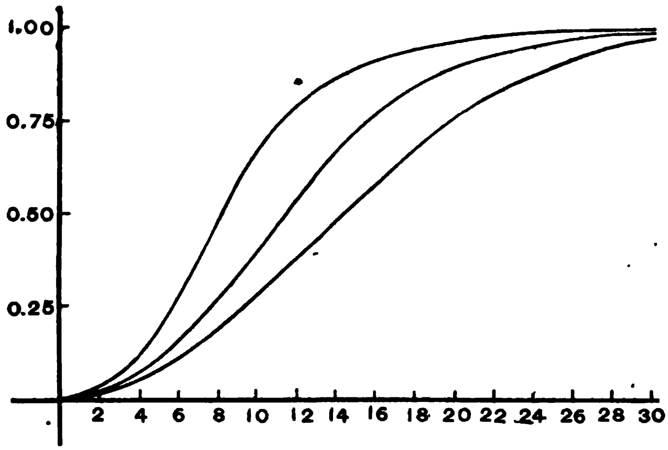

FIG. 13

There is evidently a relation between the weight of the disc and the number of times it is felt. Let the number of times be expressed as a fraction, e. g., a percentage, of the total number of experiments, and suppose the number of experiments to have been very large with the same results. Let this fraction, or percentage, be expressed in the form of a curve, where x denotes the weight of the disc and y denotes the relative frequency with which it was felt. The curve will be one of the forms shown in the accompanying figure. For an extremely sensitive person the curve will be very steep, like that to the left; for persons of less sensitiveness it will be flatter.

The experimenter is to plot the curve for his own record. The plotting is done on cross-section paper; this is paper ruled with horizontal and vertical parallel lines. On one of the horizontal lines lay off the scale of weights 2, 4, etc., with any convenient distance as the unit. On one of the vertical lines lay off a scale of percentages with any convenient distance as the unit. For each weight count upward above its place on the horizontal axis a number of spaces corresponding to its percentage; the ruling of the paper into spaces in groups of 5 and 10 make it possible to do this rapidly. Connect each dot to the following one by a straight line. The fluctuations of this line are due to irregularities in the experimenting and to the smallness of the number of experiments. The true relation of percentage to weight will be more closely indicated by a smooth line, which can either be drawn by the free hand or by adjusting a flexible ruler so as to

pass smoothly among the dots. Owing to the fact that the number of experiments was only 10 instead of an infinite number, the actual curve will differ from the theoretical one; with 100 or 1000 experiments it would approach the theoretical one more closely.

To compare the degrees of sensitiveness of different persons, two quantities can be used: either the disc that corresponds to a certain percentage, say 75%, or the percentage for a certain disc. To use either effectively a large number of experiments would be required; the calculation of the average of the two thresholds as found above gives a sufficiently accurate figure. It is evident that the higher the threshold the lower the sensitiveness; in fact, probably the only proper definition of "sensitiveness" is "the reciprocal of the threshold." "Reciprocal" of a quantity means 1 divided by that quantity. Thus, if two persons have thresholds of a' and a'' respectively, their degrees of sensitiveness will be $1/a'$ and $1/a''$.

Points to be noted.

1. Note that the uncertainty of the threshold is indicated by the size of the probable error. 2. In the curves given in the figure, y is said to be a function of x. This is expressed by $y = f(x)$. The particular curves assumed in the figure are taken from the science of probabilities.

Questions.

1. How would you define "threshold" so as to fit all kinds of sensations? 2. What are presumably some of the mental conditions of the subject that contribute to his probable error?

EXERCISE II.—SKIN SPACE.

(Needed: two æsthesiometers, millimeter scale.)

Apparatus.

In its simplest form the æsthesiometer is a pair of dividers with blunt points. The points are made of hard rubber, in order to eliminate sensations of temperature.

Experiments.

A. Open the æsthesiometer several centimeters. Touch the two points simultaneously to the cheek in a vertical direction; they will be felt as two. Repeat the experiment, reducing the distance between the points each time. To maintain the unprejudiced condition of the subject, insert occasional experiments with only one point touched to the skin. The subject is to state each time whether he feels one point or two. Con-

tinue the experiments till a mistake is made in feeling two points as one. Now apply the æsthesiometer to the scale and record the distance between the points. Repeat the measurement ten times. Find the average and average variation.

B. Repeat the experiments of *A* on the back of the neck in a vertical direction.

C. Adjust one æsthesiometer to 30^{mm} as a standard. Apply it for a moment to the cheek of the subject. Adjust the other æsthesiometer to an arbitrary distance. Apply it likewise to the cheek. The subject is to say whether the second distance was greater or less than double the first. According to the answer adjust the second æsthesiometer differently and repeat the experiment. Proceed as in *A*, the first æsthesiometer being kept at the constant distance of 30^{mm} and the second being gradually changed till a judgment of "equal to double the first" is obtained. Take ten records.

Specimen record.

A			B			C		
m	v	v^2	m	v	v^2	m	v	v^2
23	0.8	0.64	31	4.3	18.49	41	2.6	6.76
21	2.8	7.84	32	3.3	10.89	39	4.6	21.16
25	1.2	1.44	30	5.3	28.09	40	3.6	12.96
23	0.8	0.64	40	4.7	22.09	45	2.4	5.76
26	2.2	4.84	45	9.7	94.09	48	4.4	19.36
21	2.8	7.84	38	2.7	7.29	45	1.4	1.96
27	3.2	10.24	31	4.3	18.49	40	3.6	12.96
26	2.2	4.84	31	4.3	18.49	49	5.4	29.16
23	0.8	0.64	41	5.7	32.49	51	7.4	54.76
23	0.8	0.64	34	1.3	1.69	38	5.6	31.46
23.8		9)39.60	35.3		9)252.10	43.6		9)196.20
		10)4.40			10)28.01			10)21.80
		0.44			2.80			2.18

$\sqrt{0.44} = 0.66$ $\sqrt{2.80} = 1.67$ $\sqrt{2.18} = 1.48$

$\frac{2}{3} \times 0.66 = 0.44$ $\frac{2}{3} \times 16.73 = 1.11$ $\frac{2}{3} \times 1.48 = 0.99$

$a = 23.8 \pm 0.4$ $a = 35.3 \pm 1.1$ $a = 43.6 \pm 1.0$

Points to be noted.

1. Note that the results depend somewhat on the skillfulness of the experimenter. 2. Note that psychologically double does not necessarily correspond to absolutely double.

Questions.

1. What improvements would you suggest in the apparatus and the method of experimenting? 2. How would you express the relation between space on the cheek and space on the neck in your experiments?

EXERCISE III.—ARM-SPACE.

(Needed: arm-space board, cross-section paper.)

Apparatus.

In the arm-space board[1] a wooden scale carries along its upper edge a small glass rod. At the zero point in the middle there is a fixed metal plate. On each side there is a movable slide carrying an adjustable pointer. Before the experiments the pointers are pushed forward as far as possible.

Experiments.

A. The apparatus is placed on a table with the scale away from the subject. The subject, seated with eyes closed or covered, places his forefingers against the zero-plate, one on each side.

B. The experimenter moves up the two slides to the fingers till they press gently. The pointers strike the zero-plate and are pushed back automatically. This eliminates the errors due to the width of the finger, as all readings are to be taken from the end of the pointer.

C. The subject places himself directly in front of the zero-mark and closes his eyes. The experimenter places the left-hand (referring to the subject) slide at a certain distance d_1. The right-hand slide is moved out of the way. The subject moves his left fore-finger evenly outward till it strikes the slide, and then returns it to zero. The experimenter quietly moves the slide out of the way, and the subject then moves his finger again till it seems to be in the same place as before. The experimenter now moves the slide up till it touches the finger and reads the record at the end of the pointer. The tenths of a centimeter are estimated by the eye. The result in millimeters is placed in the column m_1 of the record blank.

Some other distance d_2 is now chosen and the experiment is repeated, giving a result m_2. Likewise d_3, d_4 and d_5 are used. The five distances are chosen as follows: 100, 200, 300, 400, 500 millimeters. The experiments are performed in this order: from d_1 to d_5, from d_5 to d_1, from d_5 to d_1, from d_1 to d_5. Eight complete sets are made, giving eight records for each distance. Find the averages and probable errors. Denote the averages by a_1, a_2, . . . , a_5. The difference between the given distance and the average result for that distance is the constant error of the estimate. There are thus five constant errors, $c_1 = a_1 - d_1$, $c_2 = a_2 - d_2$, . . . , $c_5 = a_5 - d_5$. The constant error expresses the average inaccuracy in reproducing the given distance. The probable error expresses the irregularity. Both these quantities depend on the values of d.

[1] New Psychology, Fig. 44.

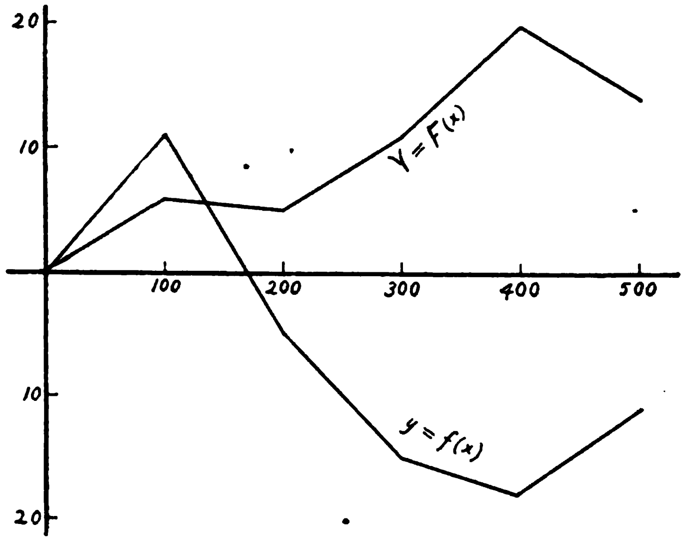

FIG. 14.

To make plain the laws of dependence, the results are to be expressed in two curves $y=f(x)$ and $Y=F(x)$ where $x=d_1, d_2, \ldots, d_5$ and y and Y are the constant errors and the probable errors. To draw these curves a certain distance is selected on the cross-section paper to represent d_1, and the points $d_1, d_2, \ldots, d_5$ are laid off on the horizontal axis. Any convenient distance is chosen for $y=1$ and the ordinates for $y_1, y_2, \ldots$ are erected. On joining the tops of these ordinates by a line the curve of results is indicated. The relation of $Y_1, Y_2, \ldots, Y_5$ to $d_1, d_2, \ldots, d_5$ is indicated in a similar manner.

Specimen record.[1]

$d_1=100$		$d_2=200$		$d_3=300$		$d_4=400$		$d_5=500$	
m_1	v_1^2	m_2	v_2^2	m_3	v_3^2	m_4	v_4^2	m_5	v_5^2
105	79.21	193	6.25	267	334.89	355	723.61	454	1253.16
116	26.01	197	2.25	281	18.49	381	0.81	494	21.16
110	0.81	189	42.25	297	136.89	399	292.41	473	268.96
107	15.21	201	30.25	285	0.09	398	259.21	505	243.36
108	8.41	195	0.25	280	28.09	371	118.81	487	5.76
115	16.81	195	0.25	269	265.69	393	123.21	500	112.36
114	9.61	196	0.25	305	388.09	408	681.21	507	309.76
112	1.21	197	2.25	298	161.29	350	1017.61	495	31.36
8)887	7)157.28	8)1564	7)84.00	8)2282	7)1333.52	8)3055	7)3216.88	8)3915	7)2245.88
110.9	8)22.47	195.5	8)12.00	285.3	8)190.39	381.0	8)459.55	489.4	8)320.84
	2.81		1.50		23.79		57.44		40.11

[1] The student need not record the residuals v but should at once write the squares v^2.

$\sqrt{2.81} = 1.68$
$\frac{2}{3} \times 1.68 = 1.12$
$a_1 = 110.9 \pm 1.1$
$c_1 = +10.9$
$r_1 = 1.1$

$\sqrt{1.50} = 1.22$
$\frac{2}{3} \times 1.22 = 0.81$
$a_2 = 195.5 \pm 0.8$
$c_2 = -4.5$
$r_2 = 0.8$

$\sqrt{23.79} = 4.88$
$\frac{2}{3} \times 4.88 = 3.25$
$a_3 = 285.3 \pm 3.3$
$c_3 = -14.7$
$r_3 = 3.3$

$\sqrt{57.44} = 7.58$
$\frac{2}{3} \times 7.58 = 5.05$
$a_4 = 381.9 \pm 5.1$
$c_4 = -18.1$
$r_4 = 5.1$

$\sqrt{40.11} = 6.33$
$\frac{2}{3} \times 6.33 = 4.22$
$a_5 = 489.4 \pm 4.2$
$c_5 = -10.6$
$r_5 = 4.2$

$c = f(d)$		$r = F(d)$	
For $d = 100$,	$c = +11$	For $d = 100$,	$r = 1.1$
$d = 200$,	$c = -7$	$d = 200$,	$r = 0.8$
$d = 300$,	$c = -15$	$d = 300$,	$r = 3.3$
$d = 400$,	$c = -18$	$d = 400$,	$r = 5.1$
$d = 500$,	$c = -11$	$d = 500$,	$r = 4.2$

Points to be noted.

1. The method of getting tenths by the eye is in this case convenient and accurate. 2. Automatic elimination of a constant error (width of the finger) from the readings. 3. Equalizing the influences of fatigue, practice and other progressive errors by changing systematically the order of the experiments.

Questions.

1. If the probable errors were directly proportional to the values of *d*, what form would the curve take? 2. What would a recorded constant error or a probable error of 0 mean?

EXERCISE IV.—MEMORY.

(Needed: two sets of geometric figures, two bands of syllables, two bands of colors, revolving cylinder, screen and metronome.)

Apparatus.

The metronome is a convenient pendulum arrangement for marking off intervals of time when great accuracy is not required. The clock-work is wound by the screw at the side; the cover is removed from the front; the pendulum is released and the weight is set at sixty. When started, the metronome will mark off seconds.

The two cards for the experiments with figures are in separate envelopes, one for each subject. The experimenter takes the envelope containing the card which he is to use on the other person as subject. The subject must not see beforehand the card that is to be used on him.

The revolving cylinder is moved by clockwork, which is kept wound by the appropriate key. It is started by releasing the brake. It revolves once in 10 seconds.[1]

One band of syllables and one band of colors will be found in each envelope with the card mentioned above.

A screen is placed before the cylinder so that only one syllable or color is seen at a time.

Experiments.

A. A pad of blank paper is placed before the subject. The experimenter holds the card with figures and at a beat of the metronome he shows it to the subject, counting 0, 1, . . . , 10 and turning down the card at 10. The subject immediately tries to reproduce on the blank paper all the figures he saw. The paper is numbered and handed to the experimenter. The card is again shown for 10 seconds and the subject again tries to reproduce the figures. This is repeated until all are reproduced correctly, unless success is not reached before the 15th trial, at which point fatigue generally begins. A record is made of how many were reproduced correctly in shape and arrangement on each trial.

B. The band of syllables is slipped on the cylinder. When it is set going it exposes one syllable per second through the screen. During the first revolution the subject calls off each syllable as he sees it; thereafter he tries to call off each syllable just before it appears, correcting himself if wrong. The experimenter notes the number of revolutions performed by the drum. This is continued until all are called off directly or until the 20th revolution. The number of revolutions is recorded.

C. The band of syllables is replaced by the band of colors and the experiment is repeated. The subject notes and recalls the colors as much as possible by visual memory and does not attempt to name them.

Points to be noted.

1. Difficulty of remembering without making external associations.
2. The prominence of motor and auditory elements in *B* and *C*.

Questions.

1. What would be some of the problems of memory that might be answered by the experiments with syllables? 2. What sources of inaccuracy do you notice in the methods of experimenting?

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[1] In case the kymograph is used, it is properly adjusted by the instructor beforehand. The study of the kymograph, which is too difficult for the student at this point, is brought forward in Exercise VIII.

EXERCISE V.—ILLUSION OF LENGTH.

(Needed: illusion board, millimeter measure.)

Apparatus.

The illusion board is made as follows. A strip of celluloid is tacked at the corners to a board 1 foot × 9 inches ($30^{cm} \times 23^{cm}$) large. The opening *ABCD* is cut in it. Six celluloid strips are prepared, such that they can be slipped under the left-hand edge of larger sheet and appear in the opening with one edge crossing at the middle *PQ*. Six somewhat longer slips are prepared, such that they can be slipped under the right-hand edge and extended past the middle under the shorter slips. The shorter slips bear diagrams of the kind shown to the left in Figure 16; the longer ones bear diagrams of the kind shown to the right. Six pairs of slips are used with diagrams of the forms indicated in the following list; the slant lines are called "angle lines."

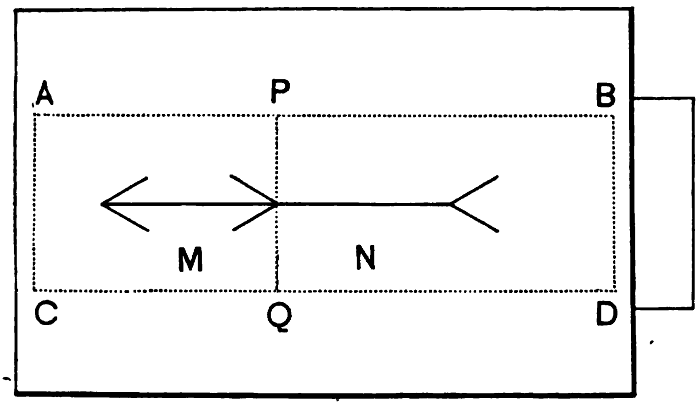

FIG. 15.

Mark on the card	*a*	*b*	*c*	*d*	*e*	*f*
Length of angle-line, in millimeters	30	30	30	15	30	60
Angle between angle-line and horizontal	15°	30°	60°	30°	30°	30°

The length of the constant horizontal line is not to be measured until all experiments are completed.

Experiments.

A_r. The slips marked *a* are inserted in the manner described. The subject holds the board directly in front perpendicularly to the line of vision at the ordinary reading distance. He moves the longer slip until the two parts of the horizontal line appear equal. He then hands the

board to the experimenter, who measures N, records the result, pulls the longer slip out slightly and hands the board back.

A_l. The experiment is repeated with N on the left-hand side.
B_r. The slips marked b are used as in A_r.
B_l. As in B_r with N to the left.
C_r. Slips c; N to right.
C_l. " c; " " left.
D_r. " d; " " right.
D_l. " d; " " left.
E_r. " e; " " right.
E_l. " e; " " left.
F_r. " f; " " right.
F_l. " f; " " left.

Each record is made in a separate column on the blanks. The experiments are performed four times in the following order: 1. A_r to F_l (as in the list above); 2. F_l to A_r; 3. F_l to A_r; 4. A_r to F_l.

Computation.

1. Find the average for each column, writing the results in whole numbers.

2. Find the averages for A, B, etc., thus:

$$A = \frac{A_r - A_l}{2},\ B = \frac{B_r - B_l}{2},\ \text{etc.}^1$$

3. Measure M.

4. Find $I_A = A - M$, $I_B = B - M$, etc. Do not disregard the sign. The results I_A, I_B, I_C give the amount of the illusion as a function of the angle, or $I = f(R)$. The results I_D, I_E, I_F give the amount of the illusion as a function of the length of the end lines, $I = F(S)$.

Points to be noted.

1. Notice carefully the system on which the experiments are arranged, in order to equalize the effects of progressive errors. 2. Notice that an orderly arrangement of the slips on the table and an appropriate routine in performing the experiments are conducive to the saving of time.

[1]Results for 13 students in 1896 were as follows:

Subject.	A	B	C	D	F	G	H	I	J	O	P	Q	S
A	90	95	83	84	71	81	84	81	81	92	90	88	82
B	97	99	83	84	75	83	86	86	85	93	93	88	86
C	97	100	90	85	79	87	90	92	88	96	92	90	83
D	96	100	84	88	78	86	90	87	85	96	95	92	85
E	96	98	79	80	75	85	87	82	83	92	92	86	80
F	97	96	80	82	69	84	86	82	84	90	92	89	80

Questions.

1. What are "progressive errors?" Mention some. 2. Why not omit the experiments under E_r and E_l and substitute in $I = f(S)$ the values from B_r and B_l? 3. How would you compute the results in order to determine the difference in the illusion between N to the right and N to the left?

EXERCISE VI.—THRESHOLD OF INTENSITY FOR SOUND.

(Needed: differential audiometer, telephone, battery of 1 A, i. e., of 1 ampére.)

Apparatus.[1]

The audiometer comprises two primary coils at the ends of a base and a movable secondary coil in the middle. The wires from the battery are brought to the binding posts at one end of the apparatus. The current passes around the one primary coil clockwise and then around the other counter-clockwise. When this current is broken by the key at the other end of the apparatus, a momentary current is aroused by each primary coil in the secondary coil.

The telephone is connected with the secondary coil. Place the secondary coil close to one of the primaries; hold the telephone to the ear and, gently moving the key, note that the induced current produces a sound. Repeat with the secondary close to the other primary.

Since the primaries are wound in opposite directions the induced currents must be in opposite directions. Consequently the sound diminishes from its maximum loudness at either end to zero at the middle.

Experiments.

A. The secondary coil is placed sufficiently near the primary to give a distinctly audible sound. The subject holds the telephone to the right ear and closes the left with the finger. He sits with his back to the apparatus.

The experimenter repeatedly interrupts the current by slightly moving the key. The subjects responds whenever he hears the click in the telephone. The secondary coil is slowly moved toward the middle until the subject first loses the sound. The sound can be regained frequently after it has been lost, but this is disregarded. The graduation is in millimeters.

B. The secondary coil is started at the middle and moved till the subject first hears the sound.

The left ear is tested in like manner.

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[1] A brief description of the simplest form of the lamp battery (see p. 77) is given at this point; the extra-circuit battery appears in Ex. XV.

Computation.

Find the averages for A and B for a set of four experiments alternated so as to equalize progressive errors.

Find the general average; denote it by t.

These values represent the faintest perceptible sound, or the threshold, under the particular conditions of the experiment.[1]

If the average value for normal individuals is T, then the subject's relative deafness can be stated as $d = \frac{t}{T}$ or his sharpness of hearing as $h = \frac{1}{d} = \frac{T}{t}$. With this particular apparatus and 1 ampère of current, $T = 200^{mm}$.

Points to be noted.

1. The scale is a purely arbitrary one. 2. The sound is very little influenced by any changes in the current.

Questions.

1. How would you define "sensitiveness to sound?" (See Ex. I). 2. What sources of error are possibly present?

EXERCISE VII.—DYNAMOMETRY OF VOLUNTARY ACTION.

(Needed: finger dynamometer, piston recorder, rubber tube, air valve, simple recording drum, smoking and varnishing arrangements, screw-vise.)

Apparatus.

a. The dynamometer.[2] This consists essentially of two spring-steel rods, rigidly fastened in a base block. These rods can be deflected inward by pressure on two small knobs. The extent of the deflection is indicated on a scale at the ends. This scale has been graduated by actual trial; the unit is the kilogram.

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[1] The following results were obtained from 16 students in 1896; the unit is the centimeter.

Subject.		A	B	D	E	F	H	I	J	K	L	M	O	P	Q	R	S	Average.
Right	*A*	22	17	21	17	25	27	17	18	26	16	44	26	12	12	11	13	20
	B	19	27	27	23	25	29	19	23	25	18	36	27	14	11	19	15	22
Left	*A*	20	17	22	18	23	22	28	19	25	17	25	17	11	11	19	14	19
	B	20	19	26	20	25	22	26	28	24	15	25	19	12	11	20	16	21

[2] See New Psychology, Figs. 4 and 24.

A glass cylinder is attached to one of the rods of the dynamometer and a rubber piston is connected with the other rod by an aluminum bar. As the rods are pressed together the air of the cylinder is forced out through a rubber tube attached to the bottom. This cylinder is called the receiving cylinder.

b. Piston recorder. At the other end of the tube is the recording cylinder, constructed similarly to the receiver. The piston of the recording cylinder is connected to an aluminum lever which is lengthened by a very light straw rod ending in a fine quill point. As the air, driven out from the receiver, is forced into the recorder, the quill point must repeat on a highly enlarged scale the movement of the rods of the dynamometer. There are various adjustments on the recorder for changing the amplification, for placing the point higher or lower, for making the plane of movement tangential to the surface of the drum, for adjusting the cylinder, etc.

Each piston is rendered air-tight by a drop of oil. The valve in the rubber tube serves to let air out or in when it is desired to change the position of the recording point.

c. Recording drum. This is a carefully turned brass cylinder, revolving on an axis. The drum is first placed with its axis horizontal. The end of a sheet of glazed paper is moistened with paste. It is then stretched tightly and smoothly around the drum and the pasted end is lapped over the other one. This makes a tight band of paper around the drum; no paste should be allowed to get on the drum itself. A gas flame is held beneath the drum so that it deposits soot on the paper; the drum is slowly turned in order to keep the paper from burning.

d. Adjusting the apparatus. The drum is placed so that its axis is vertical. The quill point of the recorder is brought near the smoked surface by moving the support and by adjusting the screws that hold it. Then the point is brought into light contact with the surface by turning the adjusting screw at the side. The lever should be as nearly as possible at a tangent to the surface of the drum. As the drum is turned, a line is drawn in the smoke by the quill point.

The dynamometer is held between the tips of the thumb and index finger; the base block rests lightly in the palm of the hand. A comfortable position is found and the eyes are closed.

Experiments.

A. Scale of effort. At the word "One" from the experimenter the subject presses the dynamometer lightly. At the word "Two" he presses it twice as hard as before; at "Three" three times as hard and at "Four" four times as hard.

When this has been done a number of times in order to familiarize the subject with the experiment, the experimenter gives the drum a slight turn before each pressure so that the records are separated distinctly. Five sets of four marks each are thus obtained.

The dynamometer is placed in a screw vise so that the cheeks of the vise take the places of the fingers of the subject. The drum is turned so that the quill point is at the first record. The vise is screwed up till the point has moved as far as the original record; the number of kilograms corresponding to this movement is read off from the scale. The tenths of a kilogram are estimated by the eye. The result is recorded in the first column of the record blank. The drum is now turned till the point is opposite the second record; the vise is screwed up and its value determined as before. The result is placed in the second column. In the same manner all the records are determined, the results being placed in the columns 1, 2, 3, 4 according to the original intention of the subject in exerting the pressures.

In each set of records the pressures were intended to stand in the relation of 1, 2, 3 and 4; the actual relations are found by dividing each record of a set by the record for the first pressure in that set. Thus the records 1.1, 1.8, 2.4, 3.2 stand in the relations of 1.0, 1.6, 2.2, 2.9. This is done separately for each set. The results for each pressure are averaged. The following is a specimen record.

Mental scale of pressure	I	II	III	IV
Actual pressure exerted	1.1	1.8	2.4	3.2
	1.8	2.1	2.8	3.7
	1.3	1.9	2.5	3.3
	1.0	1.8	2.4	3.1
	1.3	1.7	2.5	3.5
Relative pressure exerted	1.0	1.6	2.2	2.9
	1.0	1.2	1.6	2.1
	1.0	1.5	1.7	2.5
	1 0	1.8	2.4	3.1
	1.0	1.3	1.9	2.7
Average	1.0	1.5	2.0	2.7

The experiments are repeated, beginning with a very strong pressure and proceeding in the the order "Four," "Three," "Two," "One."

B. Curve of fatigue. The preparations are made as before. At the word "Go" the subject presses on the dynamometer as strongly as possible and maintains the pressure at its maximum until told to stop. The experimenter keeps the drum turning slowly for 10 sec. by the watch; thereupon he calls "Stop." The line traced upon the drum shows the

fluctuation in the maximum amount of effort. The fatigue curve is found by drawing a horizontal line from the highest point at the beginning of the record and then turning the record bottom up; the curve then runs, of course, from right to left. The amount of fatigue is to be found by taking readings at the beginning and at the end of the curve in the manner described under *A*.

C. Diversion of energy. The preparations are made as before, but instead of closing the eyes the subject keeps them fixed on a printed page. At the word "Go" he is to press as hard as possible; this maximum pressure is to be kept up without any relaxation till the end of the experiment. Shortly after starting the experimenter calls "Read" and at the same time makes a check on the drum near the quill point by means of a small stick or a pencil. The subject begins reading aloud at the signal and, without relaxing the pressure, continues to read until the words "Stop reading."[1]

D. Preserving the record. The drum is placed horizontally, the paper is slit across, one end is caught by a clamp, the sheet is run through a solution of shellac and is hung up to dry.

This shellac solution is contained in a large bottle at the varnishing stand. Lift the bottle from the lower shelf and place it on the upper one. The varnish runs through the rubber tube and floods the varnishing tray. The shellac solution is composed of 1 part by volume of saturated solution of shellac in alcohol and 4 parts of 95% alcohol. In running the sheet through the solution get the fore edge of the sheet under the solution first and keep the smoked side upward. After the sheet has been varnished, the bottle must be replaced on the lower shelf; the varnish runs back into it and is kept from evaporation.

Points to be noted.

1. Possibility of establishing mental scales. 2. The falling off in the effort in what is meant by "fatigue" in this case. To call it "the effect of fatigue" would bring in assumptions not justified by the experiment; a scientific definition must start with the facts as immediately given.

Questions.

1. How was the standard physical scale established? 2. What general conclusions would you draw concerning mental energy? 3. How many adjustments can you point out on the piston-recorder?

[1] Such a record is shown in Fig. 48 of The New Psychology.

EXERCISE VIII.—RHYTHMIC MOVEMENTS.

(Needed: JACQUET graphic chronometer, kymograph, MAREY tambours, upright wooden scale, paper, smoking and varnishing arrangements, 2 standards.)

Apparatus.

a. Graphic chronometer. This is essentially a fine stop watch with a recording point and electric contact. The smaller dial indicates seconds, the larger one minutes. The chronometer is wound by the screw at the back; it will run for 4^h without error due to laxity of spring, or 6^h with a small error. The catch *b* at the bottom, when moved to the right, starts the chronometer; when to the left, stops it. Pressure on the catch *a* at the side returns the hands to 0. The recording point *d* makes a movement once a second, or five times a second, according as the catch *c* at the back is pushed in or pulled out. The extent of the movement of the recording point is regulated by the screw *e* beneath it. When the chronometer is placed vertically the weight of the recording lever is sufficient to bring it back when moved, but when it is in a horizontal position the screw *f* at the right-hand side should be made to bear lightly on the spring at the opposite end of the lever. The chronometer is held on the support by the screw *l*. When used with the drum, it is brought near the surface at a tangent by moving the support; the finer adjustment is then made by turning the screw *m* in front.

b. Kymograph. This is a recording drum moved by clockwork. It differs from the hand-drum by having its speed so carefully regulated that, when its rate of revolution is once determined, it can be depended upon to maintain that rate with a high degree of accuracy, provided the spring is kept wound up to about the same tension and the whole apparatus is in perfect order. The lettering used in the following instructions will be found painted on the apparatus at the appropriate points.

First place the drum in the separate horizontal support as in Ex. VII. Place some paste on one end of the sheet of glazed drum-paper. Stretch the paper around the drum tightly and bring the pasted end over the other end. Coat the paper with smoke by holding a gas flame close beneath it.

Lift the drum from the support, grasping it around the ring *O* at the end. Raise the spring *G* of the kymograph by the arm *F* till it catches. Let the end of the drum-axle drop into the socket *P*. Bring the groove of the ring *O* up till it catches on the wheel at the end of the arm *N*. Bring the top of the axle just below the socket held by *G*, and let *F* snap. The drum is now in position; it should be turned till the projecting point at the bottom of the axle catches in a notch of the spring *P*. If the kymograph is not firm upon the table, adjust the leg *M*.

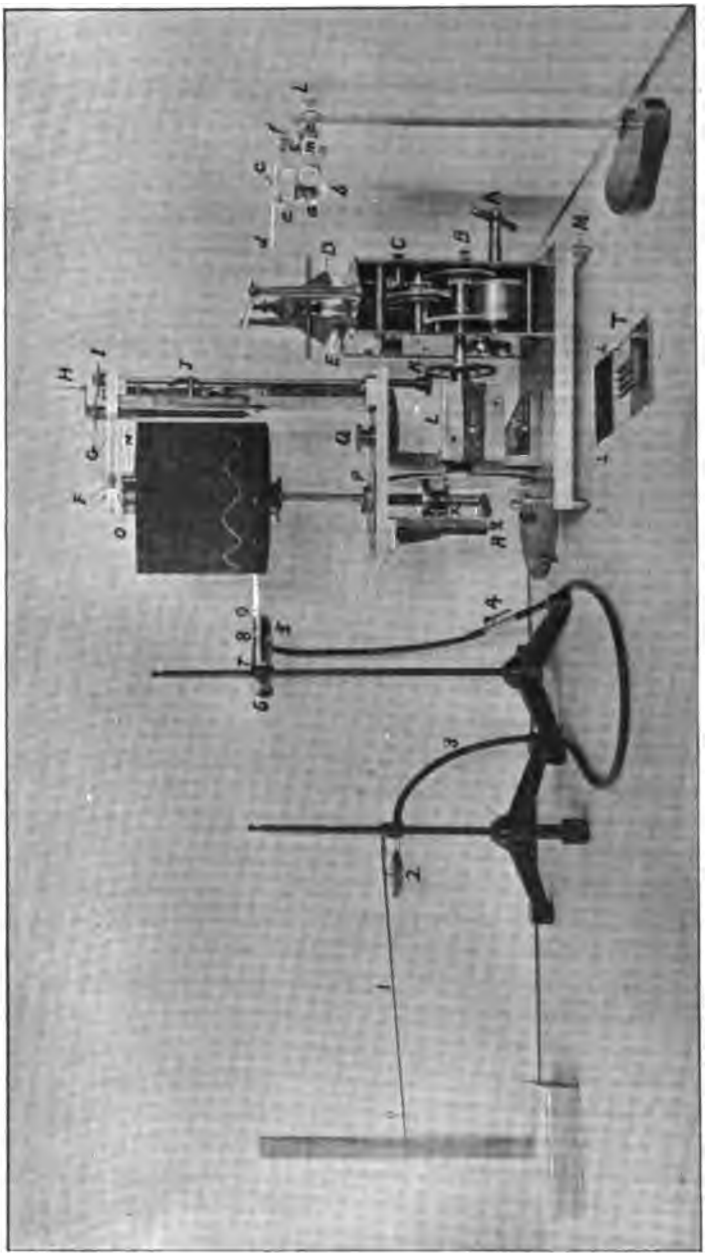

Fig. 16.

Wind up the clock-spring by the handle *A*. Move the brake *E* in order to release the governor *D*. When the screw *B* is tight the drum will turn with the clockwork; when it is loose, the drum is disconnected. The connection of the clockwork with the drum axle is established by the large friction disc which presses against the small friction roll *X*. When handling the drum, always disconnect it by turning *B*; this keeps the friction disc from being ground by chance movements of the roll *X*.

The speed of the friction disc is changed by different combinations of the gears of the clockwork. The case of the clockwork can be opened by turning two projecting screws at the side. There are two gears that move sidewise on their axles, a lower one (white stripe), and an upper one (red stripe). The following table gives the speeds approximately obtainable by the different combinations.

SPEED NAME.	POSITION OF LOWER WHEEL.	POSITION OF UPPER WHEEL.	FRICTION ROLL AT LOWEST POINT.	FRICTION ROLL AT HIGHEST POINT.
I	Left.	Right (weak spring).	$1^h\ 30^m$	12^m
II	Left.	Left (medium spring).	6^m	45^s
III	Left.	Middle (strong spring).	2^m	15^s
IV	Right.	Right (weak spring).	12^m	$1\frac{1}{2}^m$
V	Right.	Left (medium spring).	40^s	5^s
VI	Right.	Middle (strong spring).	16^s	2^s

There are three sets of springs for the governor; that set should be chosen which allows the wings of the governor to take a middle position when in motion. When the upper wheel is in the middle position the screw *C* should be turned so as to bring the little wheel at the end of the arm into position between the largest and smallest cog-wheels.

The intermediate speeds between the figures in the table are obtained by moving the roll *X* by means of the screw *R*. An index connected with *X* moves over a scale so that a speed once found can be reproduced by direct adjustment of the index to the same point; to avoid back-lash the adjustment should be made in the direction from zero upward. Adjust the kymograph for this exercise to about 20^s for 1 revolution.

c. Tambours. The essential principle of the tambour is found in its construction as a metallic air-chamber with a rubber top and a side tube. There are two tambours, the receiver and the recorder.

Any desired movement may be communicated to the straight lever (1) of the receiver (2). This lever communicates the movement to the air inside by its varying pressure on the rubber top. The movement of the air is communicated along the rubber tube (3) to the recorder (5). The

rubber top of the recorder moves in response to the movements of the air, and the light curved lever (9) resting on it consequently repeats the movements of the straight lever of the receiver. The point of connection (8) to the recording lever can be moved so as to obtain different degrees of amplification; the body of the tambour is kept centered beneath the point of connection by a screw (6) at the back. For the present exercise any convenient amplification is used. The position of the point on the drum is adjusted by the arm (7) which moves the fulcrum. The valve (4) is used as in Ex. VII.

Experiments.

A. Getting the time-line. The drum is set in motion. The graphic chronometer, adjusted to beat seconds, is placed on the upright support. The support is moved till the recording point nearly touches the smoked surface at a tangent. The point is brought into light contact by the adjusting screw. The chronometer thus traces a line with checks at intervals of 1^s. The chronometer is then carefully removed and placed in its box.

B. Recording an instinctively chosen rhythm. The recording tambour is placed so that its point draws a line on the drum. The receiver is arranged with its lever in front of a vertical scale.

The subject takes the straight lever between thumb and finger at the point marked in black. He is to move it up and down as regularly as possible over a distance of about 3^{cm}. By "regularly" is meant evenly and at regular intervals. While this is being done, the experimenter allows the drum to make one revolution. By turning the handle *H*, the drum is then lowered sufficiently for another record.

C. Recording an arbitrary rhythm. The subject tries to beat twice as fast as before.

Computation.

The drum is lifted out of the apparatus by grasping it around the ring and raising the lever *G*. It is placed in the horizontal support. The experimenter writes on it with a pointed instrument the name of the subject, the date and the time. With the point of a knife the paper is slit across, the thumb being kept over the beginning of the slit in order to keep the paper from falling. The end of the paper is caught by a varnishing clamp. The paper is run through the varnishing solution of shellac and hung up to dry.[1]

[1] Observations connected with this experiment led me to report (*Science*, 1896, n. s. IV 535), the following law, subject to amplification and correction by further experiment. The probable error (all apparatus errors being negligible) is a good measure of

When the paper is dry the lower edge containing the time-record is cut off to serve as a scale of seconds. Ten of the distances from top to top of the waves in each rhythm are measured by this scale, giving the results in seconds and estimated tenths. The average and the character-variations are found in each case.

Specimen record.

Natural rhythm.			Arbitrary rhythm.		
m	v	v^2	m	v	v^2
1.8	0.16	0.0256	1.1	0.05	0.0025
1.8	.16	.0256	0.9	.15	.0225
1.7	.06	.0036	1.0	.05	.0025
1.5	.14	.0196	1.2	.15	.0225
1.7	.06	.0036	0.8	.25	.0625
1.5	.14	.0196	1.0	.05	.0025
1.6	.04	.0016	1.1	.05	.0025
1.8	.16	.0256	1.6	.55	.3025
1.4	.24	.0576	0.9	.15	.0225
1.6	.04	.0016	0.9	.15	.0225
1.64		9)0.1840	1.05		9)0.4650
		10)0.0204			10)0.0519
		0.0020			0.0052

$\sqrt{0.0020} = 0.04$ $\qquad$ $\sqrt{0.0052} = 0.07$

$\frac{2}{3} \times 0.04 = 0.03$ $\qquad$ $\frac{2}{3} \times 0.07 = 0.05$

$a = 1.64 \pm 0.03$ $\qquad$ $a = 1.05 \pm 0.05$

Points to be noted.

1. The method of obtaining 10ths of a unit by the eye is in this case just as accurate as if a carefully prepared scale were used. 2. Although

the subject's irregularity (see above p. 21, note 1), or of the difficulty of his mental processes. Using it thus as a measure of the disadvantage of a rhythm we can express the relation of the disadvantage to the length as $r = f(t)$ where r is the probable error and t the length of the time in the rhythmic movement. The law proposed is

$$\frac{r}{|t - T|} = \text{constant},$$

where r and t have the same meanings as before, T is the length of the time chosen naturally and $|t - T|$ indicates that the sign of the quantity is disregarded. In other words, the amount of irregularity is proportional to the deviation from the natural rhythm. The relation here observed is illustrated by the well-known fact that in such rhythmic movements as walking, running, etc., a certain frequency in the repetition of the movement is most favorable to the accomplishment of work. Thus, to go the greatest distance in steady traveling day by day, the horse or the bicyclist must move his limbs with a certain frequency: not too rapid, as this would fatigue and cut short the journey, and not too slow, as this also would be fatiguing and wasteful. This favorable frequency is a particular one for each individual and for each condition in which he is found; any deviation diminishes the final result.

the records could easily be read in 100ths, the degree of accuracy appropriate to the experiment lies in 10ths.

Questions.

1. What is the relation between the probable error and regularity?
2. How would you express this relation mathematically?

EXERCISE IX.—MAXIMUM RAPIDITY OF REPEATED VOLITIONS.

(Needed: recording drum, paper, smoking and varnishing arrangements, 100 v. d. electric fork, fork support, double contact key, spark coil, condenser, battery of 1A, battery of 4A.)

Apparatus.

a. Recording drum. This drum[1] consists essentially of an evenly turned cylinder rotating on two centers. Parallel to the axis of the drum are two rails guiding an upright support, called the marker support. The support is moved by a screw operated by a handle at the end. Glazed drum-paper is stretched around the drum and smoked in the manner described in Ex. VII.

b. Electric fork. An electric current of 1 ampère is brought to the binding post at the back of the fork. It travels along the prong to the platinum wire. When this wire is in contact with the plantinum-covered disc the current passes to the base of the magnet, whence it goes through the wire of the magnet, out at the insulated post and back to the battery. While the current is passing, the coil becomes magnetic and pulls the prongs of the fork inward. If the platinum wire is in very light contact with the disc, this movement of the prongs will separate them, and the current will be interrupted. Consequently the coil ceases to be magnetic and the prongs fly outward. The outward movement, however, brings the platinum wire into contact with the disc again; the current passes through the coil, and the movement begins as before. With a proper adjustment of the disc in relation to the platinum wire the fork will continue to vibrate as long as the current is supplied. The proper adjustment is attained by starting the disc away from the wire and screwing it up till it just touches—which can be seen from the darkening of the little lamp of the battery—and then giving about ⅛ of a turn more. If the fork does not begin to vibrate of its own accord, it is

[1] See Fig. 33 in Stud. Yale Psych. Lab., 1893 I 100, and Fig. 6 in The New Psychology.

started by a light blow of the finger. A light recording point of fine spring steel is attached to one prong.[1]

c. Fork support.[2] The object of the fork support is to provide a delicate and convenient adjustment of the contact point on the smoked paper. It is fastened to the marker support of the drum by the clamp *M*, by which it can be raised, lowered or turned as desired. The rod *R* is placed in the hole through the base of the fork; the screw *S* clamps the base firmly. The screw *I* gives adjustment on the rod *P*. The fork is first placed nearly in position with its steel recording point at a tangent to the drum and its plane of vibration parallel to the drum axis. It is now lowered by the screw *N*, acting on the arm *O*, till the point is in good contact with the paper.

d. Double contact key.[3] This key consists of a lever moving on a bar hung between centers. A spring holds the front end of the key up; the tension of this spring is adjusted by the screw *Z*. It should be great enough to hold the lever up as far as it will go, but small enough to offer the least possible resistance to movement. At *Y* a platinum point is inserted. Just below *Y* there is another point *X*, which is supported by a ring of hard rubber so that there is no metallic connection with the frame. A wire leads from *X* to the binding-post 1. At the back of the lever there is a screw *W*, carrying a platinum point *V*. Just below *V* there is another point *U* supported by hard rubber and in connection with the post 2. The extent to which the lever moves is regulated by turning *W*. There should be enough movement to be distinctly felt, but not enough to cause loss of time. The framework of the key (and consequently the contacts *Y* and *V*) is connected to post 3.

Bring the wires from the 4-ampère battery to the posts 1 and 3; notice (by the darkening of the small lamp) that the current passes through the key whenever *Y* and *X* are kept in contact by pressure on the rubber knob *A*.

Bring the battery wires to 2 and 3. Notice that the current passes whenever *V* and *U* are in contact.

[1] To prepare these steel points I procure from the clock factory a flat strip of fine "pendulum wire." Pieces are cut off of 3 cm. length. A hole to fit over the screw is made in each piece; the opposite end is cut to a sharp point and bent. Such a piece is substituted for the usual brass point that comes with the fork. The great elasticity of the steel reduces the friction to a minimum; its hardness keeps the point sharp for a long time, whereby a very fine line may be produced.

[2] See above, p. 82.

[3] The lettering is found on the apparatus. A picture of such a key without the adjusting screws *Z* and *V* is given in The New Psychology, Fig. 27; post 1 is nearest, post 2 is farthest and post 3 between them.

Connect 1 and 2 by a short wire. The current is then interrupted for an instant at each movement of the key downward or upward.

e. Spark coil. The spark coil consists of two coils of wire.[1] The inner one, "primary coil," is made of a few turns of coarse wire ending in the terminals *P*. The outer one, "secondary coil," consists of many turns of very fine wire, ending in the terminals *S*. Insert a metal rod *F* through one of the posts *S* and bring its point within about $\frac{1}{2}^{mm}$ of the other post. Bring one wire of the 4A battery to one of the posts *P*. Touch the other post with the other wire for an instant; notice that a spark jumps across at *S*.

Bring one wire of the 4A battery to one of the posts *P* and the other one to the post 2 of the telegraph key. Connect post 3 with the other post *P* by an extra wire. Notice that a spark is produced every time the key knob is pressed or released.

Remove the rod *F* and connect one of the posts *S* to the metallic back of the fork by a light wire and the other one to the binding post *H* of the drum by a similar wire. The wooden base of the fork interrupts metallic connection between the two wires and the spark is forced to fly off the recording point and through the paper to the drum. This makes a white dot on the paper.[2]

f. Condenser. The condenser consists of two sets of sheets of tin-foil arranged alternately. The sets are connected to the two posts *K*, *L*. The sheets are kept separate from each other by sheets of paper. Two wires are brought from *K* and *L* to the posts 2 and 3 of the key without disturbing the battery connections. Notice that the spark at the drum is made stronger by using the condenser.

Experiments.

A. Adjustments. The fork is adjusted at the farther end of the drum. The experimenter sets the drum in motion by striking the edge with his hand. By turning the handle that moves the fork support he pulls the fork along toward him. The speed of the drum should be such that a single wave extends over about 1^{cm}. During the experiment the fork support should move fast enough to make the fork trace a spiral without overlapping, but slow enough not to waste paper. The subject adjusts the key evenly on the table. He then taps several times to prove that the spark connections are in order.

B. Recording the most rapid tapping. The subject takes a comfortable

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[1] The separable coil should be used, see above, p. 81.

[2] The plan of connection of the spark coil and fork is similar to that shown in Fig. 6 of the New Psychology.

position, grasps the knob of the key between thumb and middle finger, and, steadying the key with the other hand, makes the lever vibrate as rapidly as possible. This is done by way of practice for a few moments without making a record.

The subject is ready. The experimenter calls "Now," whereupon the subject begins to tap as rapidly as possible. The drum is set going and a record is taken for a few seconds. The name of the subject is written in the smoke by a pencil or a pointed stick.

C. Preparing the record. The paper is slit crosswise with a knife, lifted at one end and run through a solution of shellac as described in Ex. VII.

D. Computing the results. When the record is dry a portion about in the middle is selected for computation. As a dot was made at each tap and release of the key, the distance between two dots on the fork-line, or time-line, gives the time for a single movement. The time is divided into 100ths by the wave of the fork-line; the 1000ths are obtained by estimating the extra 10ths of a wave by the eye. Ten successive records are counted. The average and the probable error are found.

Specimen record.

m	v	v^2
40	0.7	0.49
39	1.7	2.89
40	0.7	0.49
42	1.3	1.69
41	0.3	0.09
43	2.3	5.29
40	0.7	0.49
39	1.7	2.89
41	0.3	0.09
42	1.3	1.69
40.7		9)16.10
		10)1.56
		0.16

$\sqrt{0.16} = 0.4 \quad \tfrac{2}{3} \times 0.4 = 0.3 \quad a = 40.7 \pm 0.3$

Theoretical considerations.

The repeated taps were produced by successive volitions resulting in the alternating movements, down and back. Assuming that the two movements represent two volitions alternated as rapidly as possible, the

result gives the average time required as a minimum for the rise and execution of a volition.[1]

Points to be noted.

1. Regularity and rapidity may be quite different in different persons.
2. A very rapid person may not be a very regular one.

Questions.

1. How are we justified in considering the time between two muscular movements as a mental time? 2. What means would you suggest for shortening the tap-time and decreasing the probable error?

Exercise X.—Simple reaction to sound.

(Needed: recording drum, automatic break, smoking and varnishing arrangements, 100 v. d. electric fork, Pfeil marker, Deprez marker, telephone, reaction key, adjustable marker support, simple break switch, two batteries of 1 ampère, one battery of 2 ampères.)[2]

Apparatus.

a. Recording drum. The drum used in this exercise is known as the "standard drum" on account of the steadiness of its movement, which is due to the weight of the wheel.[3] Glazed drum paper of the proper size is adjusted around the drum and is smoked on that quarter which is beneath the projecting arm *C*. The drum is moved by a ratchet handle at the top; it may be stopped by the brake at the side.

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[1] The following table gives the results on 20 students in 1896:

Subject:	A	B	C	E	F	G	H	I	I	J	L	M	N	O	P	Q	R	S	U	Z	Averages for all.
Average:	41	71	59	46	80	123	71	92	77	113	97	33	84	72	52	62	147	117	118	60	81
Average variation:	12	24	18	8	9	50	2	28	13	36	11	14	40	10	19	5	20	17	6	3	17
Relative average variation:	39	33	30	17	11	41	2	30	17	32	11	42	48	7	36	7	12	15	5	5	21 per cent.
Probable error:	3.4	6.8	5.1	2.2	2.5	14.0	0.6	7.8	3.6	10.1	3.1	3.9	11.2	2.8	5.3	1.4	5.6	7.8	1.7	0.8	4.5

The results give the times for the alternated movements naturally used by unpracticed persons in attempting to move the telegraph key. By trial the subject can finally select the most rapid movement, which will frequently be much quicker than the original one. The average variation (or mean variation) is the average of the residuals; in the example above it would be the average for the column *v*.

[2] The drum of the previous exercise, which is fitted with an automatic break, may be used for this exercise. The fork and adjustable marker support may likewise be the same as before. In such a case the previous exercise should be put far enough ahead of this one in the course to avoid any need for the same pieces in two simultaneous exercises. It is preferable to have several drums, all of which should have automatic contacts.

[3] See Stud. Yale Psych. Lab., 1895, III, Fig. 16.

b. Automatic break.[1] The projecting arm *C* as it passes the rubber block *D* strikes a projecting pin *G* and moves it. On the other side of *D* there is the small arm *I* made into one piece with the pin *G*; it, therefore, moves when *G* is struck. This movement brings a platinum point away from the platinum point of the screw *H*. Thus, if the two wires of an electric circuit are brought to the posts *E* and *F*, the current is interrupted every time *C* strikes *G*.

c. Marker support. The carriage *F* riding on the steel post is movable by the handle *J*. It carries a projecting rod *L*, to which forks and markers may be attached. When two markers are used, as in the present exercise, an adjustable support[2] *M* is placed on *L*. The rod of *M* is placed vertically. This rod can be rotated by the screw *N* or lever *O*.

d. Deprez marker. This marker is adjusted on the rod of the support by the screw *P*. It is so placed that when it is lengthened, by turning the screw *Q*, the fine point at the end can be brought into delicate contact with the smoked surface. Bring the two wires from a 1A battery to the posts *R*, *S*. Notice that whenever the circuit is completed the armature *V* is drawn to the magnet *U*, and that when it is broken the armature flies back. If this does not happen, the adjustment is to be made more delicate by altering the tension of the armature spring at the back or by changing the amplitude of vibration of the armature by moving the cone by means of the screw *T*.

e. Electric fork. The fork used in this exercise is arranged to vibrate just as that in Ex. IX. The wires from a battery in 1A are brought to the two binding-posts as before. The fork is not attached to the drum, but is placed on the table.

f. Pfeil marker. The PFEIL marker is placed on the adjustable support by means of its clamp in such a way that its point is downward and close to the other marker. The battery wire is removed from one post of the fork and is placed in one of the posts of the marker. A wire is then run from the other post to the fork. Whenever the current passes, the coils of the marker become magnetic and attract the armature *D*: when the current is interrupted, the armature is released. As the current is made and broken 100 times per second by the fork, the armature vibrates 100 times per second. The vibration is transmitted to the recording point by a connecting bar. The extent of the vibration is regulated by a screw *F*, which adjusts the distance of the magnets from the armature. The finer adjustment of the point against the drum is accom-

[1] The lettering is on the apparatus; the principle of the automatic break is similar to that shown in Fig. 17.

[2] See above, Fig. 10; for this exercise the clamp *M* is made of hard rubber.

plished by a screw *G* at the back. The marker is first adjusted so that its point is just above the point of the DEPREZ marker, and then the exact amount of pressure against the drum is attained by *G*. As the drum is turned, a time line is drawn whose waves each indicate 1/100 of a second.

Experiments.

A. Finding the latent time of the Deprez marker. The current is brought to one post of the automatic break, then from the other post to the DEPREZ marker and from the marker back to the battery. The drum is slowly rotated till the arm *C* opens the break and makes a check with the marker. If this is carefully done, the check marks the exact spot at which the break occurs. The carriage is then run down and back in order to draw the zero line, or the line at which the break occurs.

With the PFEIL marker in vibration the drum is now set in rapid rotation. The carriage is then moved downward with sufficient rapidity to keep the records separate. The result is a series of records, each consisting of a time-line and the line drawn by the DEPREZ marker. The paper is removed and varnished. The distance from the zero-line to the check in the marker-line gives, in terms of space, the latent time of the marker at the break; this distance is turned into time by comparison with the time-line. The time is read in 1000ths of a second. Five records are computed to find the average and the probable error.

B. Adjusting the reaction experiment. A new piece of paper is placed on the drum and smoked all around. The PFEIL marker is adjusted as before. The zero-line is made as before.

The 1A circuit through the automatic break is taken from the marker and run through a telephone instead; a click is thus produced in the telephone whenever the arm *C* strikes the point *G*. A switch for turning off the current is inserted in the circuit.

Another 1A current is run through the DEPREZ marker and then through the reaction key. This key comprises two steel rods on which run two rubber slides.[1] The adjustable slide *A* is fastened at any desired place by the nut *B*; the excursion of the movable slide *M* can thus be regulated. For the present experiment the excursion should be about 3^{mm}. The index finger is placed in the hole of *M*, the thumb is placed in the hole of *A* and the key is steadied by the third finger. The battery wires are brought to the post of *M* and the post *T* at the top. When the finger is extended, the circuit is closed; as soon as it is moved, the circuit is broken and the DEPREZ marker moves.

[1] See Stud. Yale Psych. Lab., 1893 I, Fig. 30. The lettering is on the apparatus.

The subject is comfortably seated with the reaction key in his hand; he must be so placed that he sees nothing of the experimenter's movements.

C. Performing the experiment. The subject is called to attention by telephone clicks produced by tapping the automatic break. The switch is then opened.

The drum is set in rotation; the switch is closed during one revolution, while the carriage is lowered. The telephone click is heard by the subject and the reaction is recorded by the marker. After a few turns of the drum the switch is again closed, while the carriage is lowered as before. As many records as possible are obtained on the drum. The paper is removed and varnished.

D. Computing the results. The average and the probable error are found for the reaction experiments. Since the latent time of the marker is included in the recorded time, its amount must be subtracted in order to find the reaction-time.

Specimen record.[1]

Marker.			Reaction.	
m	v	m	v	v^2
2	0	174	24.7	610.09
2	0	124	25.3	640.09
2	0	156	6.7	44.89
2	0	130	19.3	372.49
2	0	166	16.7	278.89
		141	8.3	68.89
2	0	154	4.7	22.09
		7)1045		6)2037.53
		149.3		10)339.59
				33.96

$$\sqrt{33.96} = 5.8$$
$$\tfrac{2}{3} \times 5.8 = 3.9$$

Latent time of marker, $a = 2^\sigma$, $r = 0$; Reaction-time, $a = 149^\sigma - 2^\sigma = 147^\sigma$, $r = 3.9$

[1] Results for 12 students in 1896 are given in the following table:

	Marker. Latent time.	Marker. Average variation.	Subject. Reaction time.	Subject. Average variation.
A	1	0	116	6
B	1	0	146	16
D	0	0	168	24
E	3	0	125	15
F	3	0	143	12
G	3	0	170	27
M	0	0	167	14
N	0	0	152	14
O	1	0	164	31
P	1	0	122	18
Q	2	0	107	20
R	2	0	100	61
Average:	2	0	137	21

The different latent times for the marker are due to different adjustments of the spring.

Points to be noted.

1. The determination of the reaction-time of the subject was closely analogous to that of the latent time of the marker; it would be quite justifiable to speak of the "reaction-time of the marker" or the "latent time of the subject." 2. Since the probable error of the marker was 0, the probable error for the reaction records must be a personal quantity of the subject.

Questions.

1. How would you proceed to determine the latent time for a spark coil? 2. What mental element that was measured in Ex. IX is present in the subject's reaction?

EXERCISE XI.—REGULATED RHYTHMIC ACTION..

(Needed: recording drum, motor, 100 v. d. fork, fork support, automatic make, sounder, break key, spark coil, condenser, smoking and varnishing arrangements, batteries of 1A, 2 and 3A, and 4A.)

Apparatus.

The automatic contact (Fig. 17) is attached to one of the posts of the drum by the screw *E* through the rubber block *V.* A projecting pin in the drum strikes the spring arm *B* and depresses it slightly for an instant.

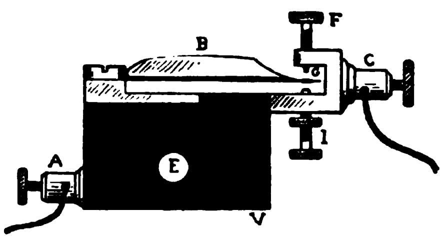

FIG. 17.

The current is brought to the post *A* and passes through the arm *B* to the platinum disc at *G*. Every time the pin on the drum strikes *B* contact is made by *G* against *I* and the circuit is closed. *F* is a screw with a rubber point against which *B* may rest. The automatic make may be turned into an automatic break by interchanging *I* and *F*.

To adjust the position of the automatic contact apparatus the screw *E*

is loosened; it is also so arranged that it can be swung entirely out of the way when not needed.

The remaining apparatus is the same as that used in Ex. VI.

The 2A current is sent through the automatic make and the sounder[1] in series.

The fork is arranged to write on the drum and to be kept vibrating as in Ex. VI.

The 4A current is sent through the break current of the key in the way explained in Ex. VI.

The motor is arranged to run the drum by a belt connecting the two pulleys. The exact rate of speed described is obtained by varying the amount of current sent through the motor.

Experiments.

A. The current through the motor is adjusted so that the drum revolves once a second. This is tested by comparing the clicks during a number of seconds with the indicated seconds of a watch. Then the subject, seated near the sounder and away from the drum, taps on the key knob in time with the clicks. A record is taken during about 15^s.

B. The drum is adjusted to revolve twice a second, and another record is made.

C. The drum is adjusted to revolve three times a second, and a record is made.

With a full-width paper on the drum all the experiments can be obtained on one sheet.

D. The zero-line is found as in Ex. VII.

E. Varnish and dry the paper.

F. Read ten successive results to the 1/1000 sec. in each record; distances to the left of the zero-line are —, to the right +.

G. Compute the constant errors (the latent time of the sounder is subtracted) and the probable errors (see Ex. III).

[1]The sounder has an electric contact arranged to close a circuit at the moment it strikes (a telegraph relay may be used). Its latent time for a current of 2 ampères can be determined and marked on it. A convenient way of determining the latent time is to connect the contact of the sounder with the wires from *C* of the battery in Fig. 7, while the wires from *G* are connected to the primary poles of the spark coil. The condenser is also attached to the primary poles. As the contact points of the sounder strike, the current which passes through *B*, *G* and the coil, is short circuited; this is practically equivalent to its being interrupted; a spark record is therefore made as usual.

Specimen record.[1]

Speed 1.			*Speed 2.*			*Speed 3.*		
m	*v*	v^2	*m*	*v*	v^2	*m*	*v*	v^2
+51	6.0	36.00	+10	7.6	57.76	−5	3.3	10.89
+39	6.0	36.	− 7	9.4	88.36	−2	0.3	0.09
+40	5.0	25.	+ 6	3.6	12.96	−3	1.3	1.69
+47	2.0	4.	+15	12.6	158.76	+1	2.7	7.29
+41	4.0	16.	−12	14.4	207.36	−3	1.3	1.69
+53	8.0	64.	+ 3	0.6	0.36	−4	2.3	5.29
+50	5.0	25.	− 1	3.4	11.56	−5	3.3	10.89
+41	4.0	16.	− 7	9.4	88.36	+1	2.7	7.29
+44	1.0	1.	+ 8	5.6	31.36	+2	3.7	13.69
+44	1.0	1.	+ 9	6.6	43.56	−1	0.7	0.49
+45.0		9)224.00	+51		9)700.40	+4		9)59.30
		10)25.00	−27		10)77.82	−23		10)6.59
		2.50	+2.4		7.78	−1.7		0.66

$\sqrt{2.50} = 1.6$	$\sqrt{7.78} = 2.8$	$\sqrt{0.66} = 0.8$
$\frac{2}{3} \times 1.6 = 1.1$	$\frac{2}{3} \times 2.8 = 1.9$	$\frac{2}{3} \times 0.8 = 0.5$
$a = +38.0 \pm 1.1$	$a = -46 \pm 1.9$	$a = -87 \pm 0.5$

Points to be noted.

1. There is a continual estimate of the time interval and an anticipatory reaction; this estimate is corrected every time by occurrence of the sound. 2. The signs of + or − for the constant errors and the largeness or smallness of the probable errors are quite different for different subjects.[2] 3. One of the speeds is specially favorable to regularity.

[1] The following table gives the results for 13 students in 1896:

	Speed 1.			*Speed 2.*			*Speed 3.*		
	Constant error.	Average variation.	Probable error.	Constant error.	Average variation.	Prob. error.	Constant error.	Average variation.	Prob. error.
A	+ 51	25	7.0	−28	9	2.5	+20	19	5.3
B	+ 44	75	21.0	−84	32	9.0	− 9	20	5.6
C	− 53	22	6.2	−23−	19	5.3	+75	17	4.8
D	+ 14	57	16.0	−42	15	4.2	+ 2	18	5.0
E	− 31	19	5.3	−30	17	4.8	−10	7	2.0
F	− 57	32	9.0	−20	10	2.8	−25	10	2.8
I	− 57	25	7.0	+36	33	9.2	−33	19	5.3
J	−105	37	10.4	−57	13	3.6	−86	20	5.6
O	+ 12	60	16.8	+10	19	5.3	+37	30	8.4
Q	+ 29	19	5.3	+10	7	2.0	+25	7	2.0
R	+ 15	20	5.6	− 8	11	3.1	+14	6	1.7
S	− 28	114	31.9	−86	16	4.5	− 5	7	2.0
T	+ 72	41	11.5	−89	83	23.2	−40	5	1 4

[2] See New Psychology, 182.

Questions.

1. How should these experiments have been performed in order to eliminate practice and fatigue? (See Ex. III, etc.) 2. A comparison of the results with each other and with those of other individuals would be likely to give some information of a person's mental constitution in regard to promptness and reliability of response; what conclusions do you draw from your record?

Exercise XII.—Simple and complex reaction-time.

(Needed: recording drum, 100 v. d. fork, fork support, multiple key, 2 condensers, reaction key, telephone, resistance box, switch, telegraph key, sounder, 3 batteries of 1A, one battery of 4A.)

Apparatus.

a. Recording drum, spark coil, condenser, fork, fork support, reaction key. See Exercises IX, X and XI.

b. Multiple key.[1] This is a key having two levers, the upper one supported on a rod with center-bearings and the lower one on the rod as an axle. They are carefully adjusted so that both rotate around the same axial line. The upper lever is held down at the back by an adjustable spring; the position and the extent of its movement are regulated by two adjusting screws that strike short upright rods. In front of the axis there are two contact screws on the upper lever. Opposite to them there are two platinum points on the lower lever. Either one of the upper screws can be made to strike the opposite point on the lower lever. If a circuit is brought to a contact screw and its opposite point, it will be closed whenever the upper lever is pressed downward. The current from a 1A battery is brought to the two binding-posts connected with such a pair of contacts, and the closing of the circuit is observed.

At the back of the lower lever there are two contact screws opposite the contact points in the base. The rearmost of these screws is turned somewhat more than the other one; it will then rest on its contact point owing to the pressure of a spring in front. The 4A current is sent through the contact. Whenever the knob of the key is pressed so that the front contact is made, the rear contact is broken. Thus the 4A circuit is broken at the moment the 1A circuit is made.

The spark coil is placed in the 4A circuit and the condenser is arranged around the break, as in Ex. IX. Observe that each pressure on the knob makes a spark on the drum.

[1] See above p. 81.

Insert the telephone in the 1A circuit. Observe that a spark is made whenever the telephone clicks.

Insert the reaction key with a condenser in the 4A circuit. Observe that a spark is made whenever the reaction occurs.

Observe that after the 4A circuit is broken by the multiple key no spark is made by the reaction key. This circuit must, therefore, be closed again after the break by the multiple key. At the front of the lower lever there is an adjustable contact point which dips into a cup of mercury covered by water. Connect the framework of the key to one of the posts for the back contact and the mercury cup to the other one. Adjust the contact point so that it is just above the surface of the mercury. Observe that as the key is depressed the 4A circuit is broken and then immediately closed again, so that it is ready for a break by the reaction key.

There are thus two sparks made; one at the moment of the telephone click and another at the moment of reaction. By laying wires from the secondary poles of the coil to the fork and the drum, as in Ex. IX, a record of the time between these two sparks is obtained.

c. Telephone, resistance and switch. The telephone has already been inserted in the 1A circuit; a shunt switch is now inserted in the same circuit. The wires are brought to the two binding posts; when the switch is closed the current can pass through the telephone, but when it is open the current cannot pass. A resistance box (or a length of resistance wire) is connected around the switch, i. e., its two poles are connected to the two posts of the switch. When the switch is now opened, the current can pass by way of the resistance, which is adjusted so that the sound from the telephone is weakened. When the switch is closed the current can pass in practically full strength through the telephone and produce a loud sound.

d. Sounder and key. An independent circuit is made to pass through a telephone sounder and a telegraph key; a pressure on the key produces a click of the sounder.

e. Arrangement. The multiple key is placed beside the drum so that the right hand can manipulate it readily while the left hand turns the handle of the marker-support on the drum. The telegraph key is placed beside the multiple key. The switch is placed just beyond.

Long wires are now inserted in the recording circuit (multiple key and reaction key), in the stimulus circuit (multiple key and telephone) and in the warning circuit (telegraph key and sounder) so that the reaction key, telephone and sounder may be taken to a distant part of the room (or another room).

Experiments.

A. Reaction with continuous expectation. The experimenter sets the drum in rotation and then taps a few times on the telegraph key. The switch is left closed. The subject holds the telephone to the ear and takes the reaction key in the hand as in Ex. X. He is to be constantly attentive and to react whenever the telephone clicks. The experimenter begins to move the marker carriage slowly and continues to do so while he presses the multiple key. Two sparks will be seen, one from the multiple key and one from the reaction key; the multiple key is then released and the movement of the carriage stopped. The experiment is repeated for about 10 times at intervals of about 15^s.

B. Reaction with specialized expectation. The experimenter proceeds as before, except by giving a warning signal on each occasion about 2½^s before the stimulus is produced. This he doos by tapping the telegraph key before pressing the multiple key. The subject is not to expect the telephone sound except when warned by the sounder. Ten experiments, as before.

C. Reaction with discrimination and choice. The subject is to react to the weaker sound and not to the louder one. The experimenter produces them in irregular order by manipulating the switch before each experiment. The procedure is as in *C.* Ten experiments.

Computation.

The record sheet is removed from the drum and varnished as in Ex. VII. The latent time of the sounder (known from Ex. XI) is allowed for as each recorded is counted. The averages and probable errors are calculated. Denote the averages by a, b and c, and the probable errors by r_a, r_b and r_c. Find $p = b - a$, $q = c - b$, $s = r_b - r_a$, and $t = r_c - r_b$. The quantity p gives the lengthening (+) or shortening (—) in the reaction-time due to the specializing of expectation; s gives the increase (+) or decrease (—) in irregularity for the same cause; q gives the lengthening due to the introduction of additional mental processes; and t gives the increase in irregularity for the same cause.

Points to be noted.

1. To eliminate progressive and other errors the phenomena *A*, *B* and *C* should be investigated in pairs; thus on one occasion *A* and *B* should be investigated, and *B* and *C* on another. 2. More mental labor is required of the subject in *A*.

Questions.

1. In determining the difference between A and B, how would you proceed in order to avoid progressive errors? 2. In determining the difference between B and C, on what system would you arrange the experiments in order to equalize differences that might arise from employing two intensities of sound?

EXERCISE XIII.—TIME ESTIMATES.

(Needed: kymograph, contact attachment, 2 MEUMANN contacts, 250 v. d. fork, fork box, 10 v. d. fork, MAREY recording tambour, induction coil, telephone, key, spark coil, condenser, plain recording point, 1A battery, 4A battery, smoking and varnishing arrangements, cross-section paper.)

Apparatus.

a. Kymograph. As in Ex. VIII.

b. Contact attachment.[1] The support is screwed to the base of the kymogragh. The projecting gear wheel is adjusted to fit to the gear wheel that has been placed on the axle of the kymograph. As the kymograph moves, the arm on the contact attachment passes over the graduated circle.

c. Meumann contact.[2] A metallic star with six arms is held to a rubber block by a screw in the center. At each rotation of the projecting arm it strikes an arm of the star and rotates it by 1/6 of a revolution. Three of the arms bear small screws which touch two metallic points sunk in the rubber block. The rubber block is fixed to the circle of the contact attachment by the projecting screw.

Let the circuit from the 1A battery be sent through the back binding-post and the left-hand side post of a contact. The circuit is completed every time an arm bearing a screw passes over the sunken contact, and is broken every time the screw arm is moved off. Consequently the central arm alternately closes and opens the circuit as it passes this block. If a current producing a tone were sent through this contact, the tone would alternately be turned on and off. A red mark on the block opposite the graduated circle serves to indicate when the circuit is closed.

The second contact is connected in series with the first one. The two side posts of this contact are connected together; the circuit is broken when the arm of the star moves, but is immediately closed again by a

[1] Fig. 236 in WUNDT, Physiol. Psychol., II 424, Leipzig, 1893.

[2] Fig. 5 in MEUMANN, *Beiträge zur Psychologie des Zeitbewusstseins*, Philos. Stud., 1896 XII 147.

contact point reaching the second sunken contact. A white mark on the block indicates when the circuit is broken.

Consequently: 1. the current passes through the second block only when it passes through the first one, i. e., at alternate revolutions of the central arm; 2. the current, when passing, is broken for a brief instant at the second block.

d. 250 v. d. fork. Same as 100 v. d. fork of Ex. IX, except in the rate of vibration, which is 250 times a second.

e. Fork box. This is a box padded with felt. Wires from the inside are brought to binding posts on the outside. The fork is put in the box and connected to the wires.

f. 10 v. d. fork. This fork, vibrating 10 times a second, is hung from a strong support. One prong carries a sliding collar connected by a small link to the metal disc on the top of a tambour. As the fork vibrates, the movement is mechanically transmitted to the air of the tambour. The amplitude of the movement of the top of the tambour is regulated by moving the collar along the prong. The tension of the rubber top is adjusted by means of the jamb nuts that clamp the tambour to the support.

g. Marey recording tambour. As in Ex. VIII.

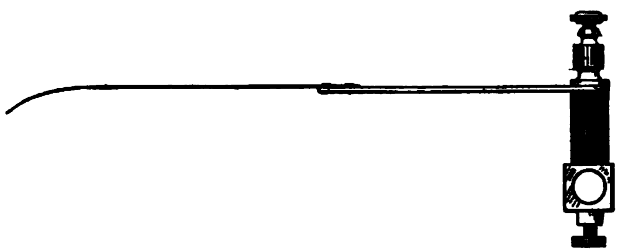

FIG. 18.

h. Induction coil. This consists of a short primary circuit of coarse wire (red) and a longer coil of thinner wire (green). The two coils are not connected; the secondary one is loose and can be placed anywhere.

i. Key. The key of Ex. IX is used; only the rear (or break) contact is employed. The condenser is connected to the key around the break.

j. Spark coil. As in Ex. IX.

k. Plain recording point. A brass arm terminated by a flexible steel point of pendulum wire is attached to a small rubber block by a thumb screw (Fig. 18). The opening through which the screw passes is elongated to allow adjustment of the point lengthwise. During the experiment the point is adjusted to lightly touch the surface of the drum.

l. Smoking and varnishing. As in Ex. VII.

m. Timing the drum. The MAREY tambour is adjusted so as to write on the drum. The tube is connected with the tambour of the 10 v. d. fork. A blow is struck on one prong of the fork and the tambour-point vibrates 10 times a second. As the drum revolves, the time line is drawn on it. Adjust the drum to revolve once in 9 seconds. [1]

n. Adjustment of the tone. Bring one of the wires from the 1A battery to the fork by way of the fork box und from the fork through the primary circuit of the induction coil back to the battery. Connect the ends of the secondary coil to the telephone and lay the coil beside the primary one. A tone is heard in the telephone when the fork is set vibrating.

Break into the telephone circuit and by extra wires insert the star contacts (which have already been connected together as described under *c*). When the kymograph is running, a tone will be turned on and then interrupted for an instant; this occurs at alternate revolutions.

o. Adjusting the spark record. The current from the 4A battery is run through the break key and through the primary circuit of the spark coil. The condenser must be connected around the break. The simple recording point is connected to one of the secondary poles, the drum to the other. Whenever the key is tapped, a spark flies from the point to the drum.

p. Drawing the zero-lines. Place the first contact with its red mark at 0°. Loosen the back screw *B* (see Fig. 17) of the kymograph. Turn the drum slowly till the tone is first heard. Draw a zero-line by lowering the drum axially by the handle *H*. This line indicates the moment at which the tone begins. With the second contact at 40° turn the drum further until the tone has been interrupted and just begun again; draw another zero-line, which indicates the moment at which the second tone begins. Place the second contact at 80° and then at 120° and draw zero-lines as before.

Experiments.

A. Measuring the time estimate for a tone of one second. Place the contacts so that the red and white indicating points are 40° apart. This will give a tone lasting one second, then a moment's interruption (1/8 of a sec.) and thereafter another tone.

The subject is seated with eyes closed. The kymograph is set in motion. The tone begins, is interrupted for an instant and is begun again. When the subject thinks that it has lasted as long after the

[1] If desired, the drum may be timed by a watch, by the method used in Ex. VIII. or by an electric fork.

interruption as it did before, he presses the key. After a few preliminary experiments for the sake of practice, the experimenter lowers the drum slightly (by the small handle at the top) after each experiment. Six records are taken.

B. Measuring the time estimate for two seconds. The contacts are placed at 80° apart and the experiments are repeated. Six records.

C. Measuring the time estimate for three seconds. The contacts are placed 120° apart. Six records.

D. Measuring the time estimate for four seconds. The contacts are placed 160° apart. Six records.

Computation.

Cut off a piece of the time-line. Use it as a scale, reading the 10ths of a second and estimating the 100ths. The records for 1 second are placed in the first column, those for 2 seconds in the fourth column, etc. The average is found for each column. The probable errors are also computed.

Let the standard interval be indicated by T and the interval as estimated by t. Find the constant error $c = t - T$ for each value of T. Thus for $T = 1$ sec. and $t = 85$, $c = 85 - 100 = -15$.

In this way two series of values, the constant error c and the probable error r, are obtained for the series of values used for T[1]. Put $c = f(T)$ and $r = F(T)$, as explained in Ex. III. Plot the curves for these functions as explained in Ex. I. and II.

Points to be noted.

1. Note the tendency to underestimate long intervals of time. Extremely short intervals are overestimated. 2. The reaction-time is not

[1] Results for 9 students in 1896 are given in the following table. In this case the time was measured from the beginning of the first tone to the beginning of the second.

	1 Sec.			2 Sec.			3 Sec.			4 Sec.		
	Const. error.	Aver. var.	Prob. error.	Const. error.	Aver. var.	Prob. error.	Const. error.	Aver. var.	Prob. error.	Const. error.	Aver. var.	Prob. error.
C	+17	13	3.6	+69	8	2.2	+81	20	5.6	+ 84	26	7.3
G	+21	13	3.6	—28	20	5.6	—73	12	3.4	—109	15	4.2
I	—23	10	2.8	—22	36	10.1	—57	50	14.0	— 86	40	11.2
K	+ 1	9	2.5	—52	9	2.5	—70	20	5.6	— 49	18	5.0
O	—66	4	1.1	—88	10	2.8	—28	22	6.2	— 50	28	7.8
P	—23	8	2.2	+41	27	7.6	—63	21	5.9	—135	42	11.8
R	— 7	11	3.1	+27	15	4.2	+ 3	12	3.4	— 7	18	5.0
S	+25	14	3.9	+11	28	7.8	—87	8	2.2	—149	18	5.0
T	—13	12	3.4	—92	21	5.9	+35	62	17.4	—104	37	10.4

to be subtracted from the second interval, because the subject involuntarily times his movement to occur at the point desired; see Ex. XI.

Questions.

1. These experiments are frequently referred to as being on the "time-sense" or "time-consciousness." How would you define such a term scientifically? Compare Ex. IV (Point 2). 2. What mental characteristics would be indicated by a large negative constant error and a large probable error?

EXERCISE XIV—COMPLEX REACTION-TIME.

(Needed: pendulum chronoscope.[1])

Apparatus.

An accurately adjusted double-bob pendulum is held by a catch at the right-hand side. The light pointer is caught on the projecting arm of the pendulum.

The chronoscope is first leveled by placing a spirit level on the base in a line parallel to the front; the screws of the two fore-legs are turned till the bubble of the level is in the middle. The level is then turned at right angles, and the screw of the rear leg is turned.

The large milled head in front is turned clockwise as far as it will go, thereby closing the shutter and setting the reaction button at the back.

When the experimenter presses the release at the right, the pendulum swings forward, releasing the shutter by striking a pin opposite the place where the pointer indicates zero. When the subject presses the reaction button, a horizontal bar behind the pointer clamps it to the scale. The graduation on the scale gives the time between the fall of the shutter and the pressing of the button. This scale is established by direct comparison with fork records on the drum in a manner similar to methods employed in Ex. VII. The figures on the scale indicate 100ths of a second; in this exercise the readings are to be in 100ths, not 1000ths.

In executing an experiment the pendulum is placed at the right, the pointer is carefully caught on it, and the milled head is turned. Red and white slips are inserted alternately in the vertical exposure wheel. The subject is comfortably seated with his finger on the reaction button; he is to press the button as soon as the shutter drops. The pendulum is released and the experiment is made; this is repeated until familiarity with apparatus and method is attained. The experimenter says "Now" about two seconds before each experiment.

[1] See Stud. Yale Psych. Lab., 1895, III 98, and New Psychology, Ch. IX.

The individual experiments should be separated by about 10 seconds; the groups by at least 30 seconds. When a group is begun, three or four experiments should be first taken without making any record of them. The subject must not know his results.

Experiments.

A. Simple reaction. The exposure wheel is turned so that a red slip is behind the shutter. The subject knows the color to be shown; he is to press the button just as soon he sees it. Five records are taken; the results are placed in the column m_1.

B. Reaction with discrimination. The subject is to react every time as before, but is to see the color distinctly before reacting. The experimenter at each experiment gives the exposure wheel a twirl, letting it stop on whichever notch it happens to strike. If it stops between two colors it is turned to the next notch. The presentation of the colors is thus a matter of chance. Five records are made; column m_2.

C. Reaction with discrimination and choice. The subject is to react only to red; for white he is to remain still. The wheel is twirled as before. Five records are made. An account is also kept of the number of mistakes; column m_3.

D. Same as *C*, but with reaction to white and rest to red column m_3.

E. Same as *B*; column m_2.

F. Same as *A*, but with white instead of ~~red; column~~ m_1.

The simple reaction time is composed of two mental processes, sensation and volition. The extra process introduced in experiments *C.* and *E.* is known as discrimination; the average for m_1 substracted from that for m_2 will give the discrimination time. The process introduced in *B.* and *D.* is known as choice; the average for m_2 from that for m_3 will give the time of choice.

Specimen record.

m_1	v_1	v_1^2	m_2	v_2	v_2^2	m_3	v_3	v_3^2
21	0.1	0.01	29	0.1	0.01	35	4.9	24.01
22	0.9	0.81	30	1.1	1.21	34	5.9	34.81
20	1.1	1.21	29	0.1	0.01	39	0.9	0.81
22	0.9	0.81	28	0.9	0.81	41	1.1	1.21
23	1.9	3.61	27	2.9	8.41	32	7.9	62.41
21	0.1	0.01	29	0.1	0.01	42	2.1	4.41
21	0.1	0.01	32	3.1	9.61	37	2.9	8.41
20	1.1	1.21	35	6.1	37.21	35	4.9	14.01
20	1.1	1.21	29	0.1	0.01	30	9.9	98.01
21	0.1	0.01	28	0.9	0.81	44	5.1	26.01
21.1		9)8.90	28.9		9)58.10	39.9		9)284.10
		10)0.99			10)6.46			10)31.57
		0.09			0.65			3.16

$\sqrt{0.09} = 0.3$ $\sqrt{0.65} = 0.8$ $\sqrt{3.16} = 1.8$

$\frac{2}{3} \times 0.3 = 0.2$ $\frac{2}{3} \times 0.8 = 0.5$ $\frac{2}{3} \times 1.8 = 1.2$

Simple reaction-time : $a_1 = 21.1$, $r_1 = 0.2$.

Reaction with discrimination : $a_2 = 28.9$, $r_2 = 0.5$.

Reaction with discrimination and choice : $a_3 = 39.9$, $r_3 = 1.2$.

Discrimination-time, $d = a_2 - a_1 = 7.8$.

Choice-time, $c = a_3 - a_2 = 11.0$.

Points to be noted.

1. The empirically established scale takes up the errors of the apparatus. 2. The results d and c are termed "discrimination-time" and "choice-time;" these terms are to be defined by giving the manner in which the results were obtained. They are not to be defined as the times required for the execution of two processes known as discrimination and choice which are defined in some other way. 2. We would expect that $a_3 - a_1 = d + c$, but this will rarely happen in performing the exercise owing to unavoidable sources of error in the untrained subject.

Questions.

1. What would probably have been the change in the results if the experimenter had arbitrarily placed the colors instead of allowing the selection by chance and if the subject had known this fact? 2. Why is the probable error larger for a_3 than for a_2?

EXERCISE XV.—ASSOCIATION-TIME.

(Needed: pendulum chronoscope, cards with words, chin key, 2A extra-circuit battery.)

Apparatus.

a. Pendulum chronoscope. See Ex. XIII. An electromagnet beneath the base is arranged to operate the reaction-key whenever the current is sent through it. The poles are at the posts marked *C* and *D*.

b. Cards with words. Two sets of small cards for the exposure wheel are placed beside the apparatus. One set is to be used by each experimenter; it must not be seen by the subject; therefore the boxes containing the sets are not to be opened until everything has been arranged for taking records.

c. Chin key. This is the modified telegraph key of Ex. IX., arranged for a break contact only, as in Ex. XII. It is mounted on blocks so as to be just below the chin of the subject. The current from the 2A battery is brought to the posts *C*, *D*. This causes the straight bar behind the chronoscope scale to clamp the pointer.

The back contact of the key is now similarly connected with the battery. As contact is made as long as the key is untouched, this acts as a "shunt" and the current passes through the key (almost entirely) rather than through the longer and more difficult circuit of the magnet. Consequently the magnet does not act until the key is touched.[1]

The subject is comfortably seated so as to see the exposure opening. The key is adjusted to bear lightly against his chin.

Experiments.

A. Sensory motor association. The experimenter opens the box containing the cards, and, out of sight of the subject, inserts one of them in the exposure wheel. When the shutter falls and exposes the word the subject is to repeat the word aloud, always emphasizing the chin movement so as to move the key knob. Three or four experiments are taken in order to give practice. Thereafter the wheel is filled with 10 new cards. Five records are made.

B. Association of ideas. Leaving the same cards in the wheel the subect upon seeing a word is to call out the first other word that occurs to him. Several experiments are made for practice. Ten new cards are then inserted and five records are taken. After each record the pair of associated words is written.

C. Fill the wheel with ten cards more and make five records on association of ideas as in *B.*

D. Fill the wheel with ten cards more and make five records on sensory motor association as in *A.*

Computation.

Find the average and the probable error for each kind of association.

If we assume that in the association of ideas an extra mental process is directly added to the sensory motor association, the time for this process is found by subtracting the time for sensory motor association from the gross association-time as recorded.

Points to be noted.

1. Note that the larger characteristic variations, as compared with Ex. XIII, may indicate any or all of the following sources: 1. complexity of the processes; 2. presence of disturbing influences; 3. vagueness in the definitions; 4. inadequacy of the methods of experimenting.

2. Note that the subject is not told to associate as quickly as possible.

[1] With a lamp battery the connections are made as in Fig. 7; the wires from socket *G* are brought to the magnet and those from socket *C* to the key.

Questions.

1. What is the relation between simple reaction and sensory motor association? 2. Why is the latent time of the magnet left undetermined?

EXERCISE XVI.—REPRODUCTION OF A TONE BY THE VOICE.

(Needed: 100 v. d. fork, spark coil, condenser, PULUJ tube, simple switch, disc with graduated series of dots, manometric flame with mouthpiece, motor with lamp board, 4A battery.)

Apparatus.

a. Electric fork. The current from the 4A battery is run through the fork in a manner similar to that for the battery through the fork in Ex. IX. To diminish the spark the poles of the condenser (Ex. IX) are connected to the fork at each side of the break, i. e., at the points closest to the platinum wire and the platinum disc. One of these points is the binding post at the back of the fork (it could not be closer without interfering with the vibration of the fork); the other is at a screw on the brass base supporting the platinun disc (not at the binding post at the end of the magnet wire).

b. Spark coil and Puluj tube. One of the wires is removed from the fork and brought to one of the primary poles *P* of the coil (Ex. IX); the other pole *P* is connected to the fork. The current thus runs through the primary circuit of the coil; as it is interrupted by the fork 100 times per second, 100 sparks occur at the poles of the secondary coil.

The poles of the secondary coil are connected to the posts of a shunt switch (Ex. XII). Thread-like wires are led from this key to the poles of the PULUJ tube.

The PULUJ tube is a vacuum tube having between the electrodes a mica plate coated with a phosphorescent substance. When the short circuiting key is open, this surface gives a flash of light at every break in the primary circuit; consequently, when the fork is vibrating, it flashes 100 times a second. If the flash is not strong, reverse the wires at the poles.

c. Disc with dot scale. A disc of cardboard is marked with 21 rows of dots, each row differing from the next by one dot.[1] The disc is placed on the axle of a motor, to which the current is supplied by a lamp-board, When the disc is put in rotation the dots fuse into a set of gray rings. The room is darkened and the PULUJ tube is placed close to the disc so as to illuminate it. At a certain speed of the disc one of the rings will reappear as a series of dots at rest; according to the laws of stroboscopic

[1] Fig. 19 is a reduced copy of the original disc which is 75 cm. in diameter. Although rather small, Fig. 19 can be cut out and used on the motor.

vision there must then be 100 dots in that ring passing by the tube in one second. The neighboring rings will also break up into dots, but the dots will appear in motion; those in which less than 100 pass per second will appear to move backward, while those in which there are more will move forward. It will sometimes happen that no ring will pass exactly 100 dots; a touch on the axle will then slightly diminish the speed. The speed of the motor is adjusted by varying the amount of current.

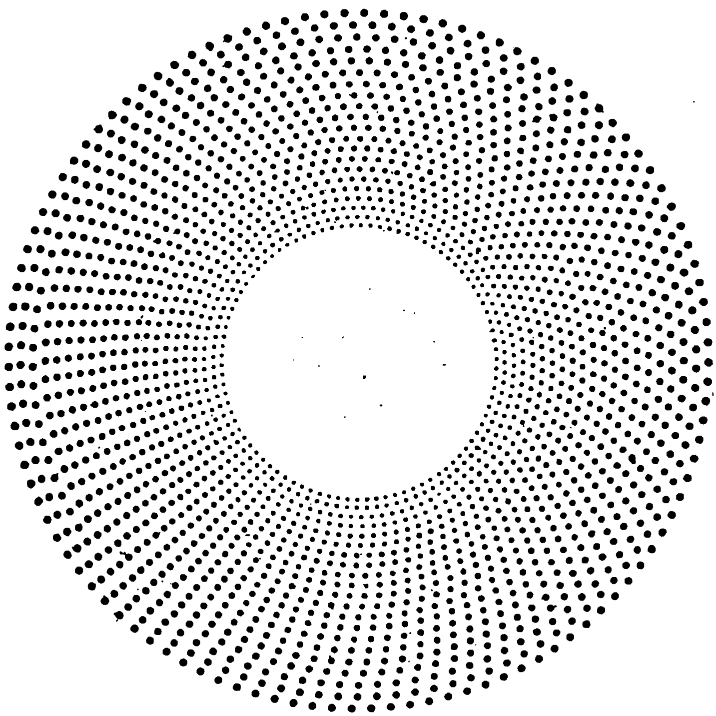

FIG. 19.

d. Manometric flame. Illuminating gas is brought to the front part of a capsule (Fig. 20) from which it issues as a small jet; the size of this jet is regulated by a small stopcock. The back of this capsule is formed by a thin membrane. Back of this membrane is a small chamber opening into a trumpet-shaped mouthpiece. Upon singing into this mouthpiece the membrane is made to vibrate by the vibration of the air producing the tone. The gas in the capsule is likewise set in vibration, whereby the flame alternates between maximum and minimum of size with every vibra-

tion. These vibrations of the flame can be seen by moving the eyes suddenly sidewise. The vibrations produce periods of light and darkness just as in the case of the PULUJ tube. Thus if a tone of 100 vibrations be sung, there will be 100 flashes per second. By holding the flame close to the rotating disc that series of dots can be picked out in which the number of dots passing corresponds to the number of vibrations in the flame.

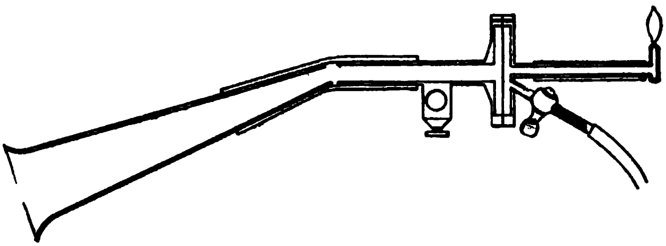

FIG. 20.

Experiments.

The experimenter is to pick out the 100 row of dots, i. e., the one which appears to remain motionless when illuminated by the PULUJ tube. The subject, holding the capsule in his hand, is to sing the same tone as the fork. While doing this he is to move the flame along in front of the disc until he finds the row of dots showing the number of vibrations in the tone sung. The experimenter records the number of rows by which this one differs from the correct one; toward the center is —, toward the outside +. A difference of 1 row means an error of 1 vibration in singing the tone.

The octave of the fork-tone is sung and the error is noted as before. A difference of one row means an error of two vibrations.

Ten records are made on each point. Averages and characteristic variations are calculated.

EXERCISE XVII.—COLOR LAG.

(Needed: electric color wheel with speed indicator, black and red MAXWELL discs, backing disc, circular scale and paper ring for discs, small telescope on GAUSS tripod, battery of 2A, simple switch, 32 c. p. lamp, tape measure.)

Apparatus.[1]

a. Color discs. A red and black disc are slipped together by means of the radial slits. The backing disc is an unslit one which is placed

[1] See Stud. Yale Psych. Lab., 1895 III 102, Fig. 17, for a view of the discs, motor, speed indicator and connections.

behind the others in order to keep the edges from flapping when they are rotated.

b. Color wheel. The essential is a rapidly rotating axle with a screw-nut for fastening the discs against a flange. This is best obtained by using an electric motor. For the present exercise the motor is series-wound in order that its speed may be controlled by the amount of current. The current is brought to the two posts of the motor, passing in its way the two poles of a shunt switch. When the switch is closed the current passes directly across without going through the motor; when it is open the current is forced to pass through the motor.

c. Speed indicator. The rotation of the motor axle causes the indicator arms to revolve and to fly outward. The extent to which they move outward depends on the relation of the speed of revolution to the weight of the arms and the tension of the restraining springs. A pointer is connected with the indicator to show just how much the arms move. The pointer moves over a scale graduated to show the number of revolutions per second. The scale was established empirically by spark-records on the drum, after the manner of Exercise IX, the place of the key in that exercise being taken by a revolving pin at the end of the axle striking against a metal spring.

d. Telescope. This may be a simple tube blackened inside; it limits the amount of the disc seen to a circle of definite area. Or a simple reading telescope of the usual kind may be used. It is placed on the tripod.

e. Adjusting the apparatus. The nut is removed from the color wheel. The backing disc is placed on the axle. The red and black discs are slipped together, so that when rotated they will not catch the wind; if the motor rotates counter-clockwise the edges must overlap to the right. The discs thus slipped together are placed on the axle, a small paper ring is placed in front and the nut is partly screwed up. The circular graduated scale is laid over the discs and they are moved till each occupies half the circle. The nut is then screwed tight.

The battery is turned on and the motor started.

The 32-c. p. lamp is lighted and placed at ½ meter in front and slightly below the discs.

The GAUSS tripod is placed at a distance of 1 meter from the motor. The telescope is then adjusted so that the eye sees the whole field of view as a colored circle.

Experiments.

The motor being at rest and the subject looking through the telescope, the experimenter says "Ready" and sends the current through the motor.

The subject sees the color alternate with black, at first slowly, then rapidly. Soon they appear to mix, but still retain a flickering appearance. As the speed continues to increase, the flickering becomes less marked and finally diappears. At the moment when the illumination of the field appears to be constant and steady the subject says "Now." The experimenter notes and records the position of the pointer at this moment.

The speed of the motor is allowed to increase considerably beyond the point just recorded; the current is then turned off. The speed gradually decreases and at some point the illuminated field begins to flicker. At this point the subject says "Now," and the experimenter notes and records the position of the pointer.

Records of the first kind may be called "up-records," those of the other "down-records." The up-records are placed in one column, the down-records in another.

After a few preliminary experiments to obtain practice, ten records are made of each kind.

Computation.

Let m be the average of the up-records and n that of the down-records. The time of one revolution in seconds is thus $1/m$, or $1/n$, and the time of half a revolution in $\frac{1}{2}m$ or $\frac{1}{2}n$.

Let the suppositions be made: 1, that the black was equivalent to absence of light; 2, that there was no lag at the appearance of the red. It follows that since the red seemed to be present all the time, whereas it was present only half the time, the sensation must have persisted through the time of $\frac{1}{2}$ a revolution, i. e., $2m$ or $2n$ sec., without any perceptible diminution of intensity.

The suppositions are not strictly according to fact. The black of the disc is not an absolute black, and there is a small lag at the beginning of the red sensation. For a bright red and a cloth black these errors may be neglected, as in the present case.

Below the records the following statements are to be made (all figures being in decimals to the 1/100 and the unit being seconds): lag, up, = ; lag, down, = ; lag, average, = .

Points to be noted.

1. The lag may be dependent on the intensity of the light and the area of the field. 2. The lag is a psychical and not a physical affair.

Questions.

1. Would you add or subtract the latent time (or lag of the color at the beginning), if it were known and you wished the true time of lag? Why? 2. What is the unit of measurement in this exercise?

NOTES.

As this number of the Studies has, for various reasons, been delayed beyond its usual time, it was considered advisable to bring references, etc., down to 1897.

The following list will supply the information, not previously given, concerning the occasions of the various articles published in the Studies. An article as published is condensed and corrected from the original thesis.

1. Theses for the degree of Ph.D.: *Investigation in reaction-time and attention*, by C. B. BLISS; *Researches on the mental and physical development of school-children*, by J. A. GILBERT; *Measurements of illusions and hallucinations in normal life*, by C. E. SEASHORE; *Studies of fatigue*, by JOHN M. MOORE.

2. Theses for the degree of M.D., the work being done under the direction of the Psychological Laboratory and presented to the faculty of the Yale Medical School: *On the relation of the reaction-time to variations in intensity and pitch of the stimulus*, by M. D. SLATTERY; *Reaction-time in abnormal conditions of the nervous system*, by ALFRED G. NADLER; *Simple and cortical reaction-time*, by HOWARD F. SMITH.

3. Theses for special honors at graduation from Yale College, the theses being frequently made parts of larger articles: *Experiments on the highest audible tone*, by HOWARD F. SMITH; *Some experiments on the reaction-time of a dog*, by EDWARD M. WEYER; *Researches on reaction-time*, by JOHN L. BURNHAM; *Researches on reaction-time*, by A. E. VON TOBEL; *Researches on reaction-time*, by A. SILVERSTEIN; *Researches on reaction-time*, by G. R. HOLDEN; *Researches on voluntary effort*, by H. R. MCDERMOTT.

The regular courses given each year in the laboratory are as in the following list. The amount of direct personal supervision over the work of a student in a course can be roughly inferred from the total number in that course; the total for the present year is stated after each course.

1. *Physiological and Experimental Psychology.* Two lectures per week throughout the year. The material covered by the demonstrations and experiments is about that contained in Ladd's Outlines of Physiological Psychology and Scripture's New Psychology. 127 seniors and juniors (elective), 8 graduates, 3 specials.

2. *Elementary Laboratory Practice.* One exercise of two hours per week throughout the year. See above p. 89. 3 seniors and juniors, 7 graduates.

3. *Advanced Course in Experimental Psychology.* One lecture and one exercise per week throughout the year. Elements of analytical geometry and calculus with illustra-

tions from psychology; theory of probabilities; statistics; theory of measurements; practice in adjusting measurements; technical training in the construction and care of apparatus; principles of laboratory economy; methods of experimental instruction; practice in the use of the lantern, etc. 6 graduates.

4. *Educational Psychology.* One hour per week throughout a year. Application of modern psychological principles to educational subjects; outlines of the psychology of touch, its use in education; motor abilities, accuracy of movement, fundamental principles of writing and drawing; sight, color-teaching; space, form-teaching, drawing, modeling; attention, concentration and distraction, laws for developing attention; memory, analysis into its components, experimental study of, calculation of results, development and training, time of study; imagination, use, necessity of development and repression, fables, children's books, toys; emotions, will, action, reflex, automatic, instinctive, voluntary, their training; education of the blind, the deaf and other defectives; child-study, principles of anthropometry and psychometry; psychological development of the child, beginnings of instruction; economy in education, greatest results from least efforts, correlation and concentration of instruction; various educational subjects from a psychological standpoint,—amusement, play, toys, picture-books, object-lessons, etc. The course is illustrated with experiments, lantern views, and a large collection of educational material from Europe and America. 10 seniors and juniors, 2 graduates.

5. *Research-Work in Psychology.* Participants in this course are either investigators or assistants. For assistants the object is such a training in accurate introspection, observation, experimenting and the art of research as is desirable for the general psychologist. This work is open to all. Only those who have had sufficient experience are permitted to undertake independent investigations. The results of all investigations belong to the archives of the laboratory. Those who undertake investigations thereby agree to prepare the results for publication, subject to approval, in the *Studies from the Yale Psychological Laboratory.* 1 senior, 4 graduates.

STUDIES

FROM THE

Yale Psychological Laboratory

EDITED BY

EDWARD W. SCRIPTURE, PH.D.

Director of the Laboratory of the Department of Philosophy.

Volume V

1897

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RESEARCHES ON ACOUSTIC SPACE.[1]

BY

MATATARO MATSUMOTO.

E. WEBER[2] seems to have been the first to call attention to the errors in localizing sounds. The particular problems involved seem to be two: 1. the perception of the direction from which a sound comes; and 2. the perception of its distance. The investigations described in the following pages were made in the attempt to contribute data toward the solution of these two problems. The work was begun (1894) in the Psychological Laboratory of the Imperial University of Japan (Tokyo) at the suggestion of Professor MOTORA; the greater part of the work, however, was done during the years 1896–1898 in the Psychological Laboratory of Yale University under the supervision of its director, E. W. SCRIPTURE. Many suggestions were also received from Professor LADD.

I. PRELIMINARY INVESTIGATIONS.

The first series of experiments was conducted according to PREYER'S statistical method.[3] Instead of PREYER'S sound-helmet, a hollow spherical cage was devised as is shown in Figure 1. The imaginary surface of the sphere whose diameter is 1.35^{m} is divided into 8 equal parts by 4 vertical great circles. The surface is again divided horizontally by the equator and by two small circles parallel to the equator at a distance of 45° from poles. The intersecting points of these vertical and horizontal circles correspond to the 26 terminal points of 13 axes or diameters of the sphere. These 13 axes may be divided into three classes.

I. Three primary axes which cut each other at right angles.

(*a*) The frontal axis, or the diameter of the sphere from right to left in the plane of the equator. As this line corresponds to an imaginary line

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[1] Submitted to the Tokyo Imperial University as a thesis for the degree of Hakush (Ph.D.).

[2] ED. WEBER, *Ueber den Mechanismus des Gehörorgans*, Ber. d. kgl.-sächs. Ges. der Wiss., math.-phys. Classe, 1851, 29.

[3] PREYER, *Die Wahrnehmung der Schallrichtung mittelst der Bogengänge*, Archiv f. d. ges. Physiol. (Pflüger), 1887 XL 586.

drawn through the external openings of the two ears of the subject seated in the cage, it may be called the auditory axis or the *rl* (right-left) axis.

(*b*) A vertical diameter which intersects the frontal axis at its middle point. This may be called the vertical or the *ou* (over-under) axis.

The plane determined by the two axes *rl* and *ou* is called the frontal plane.

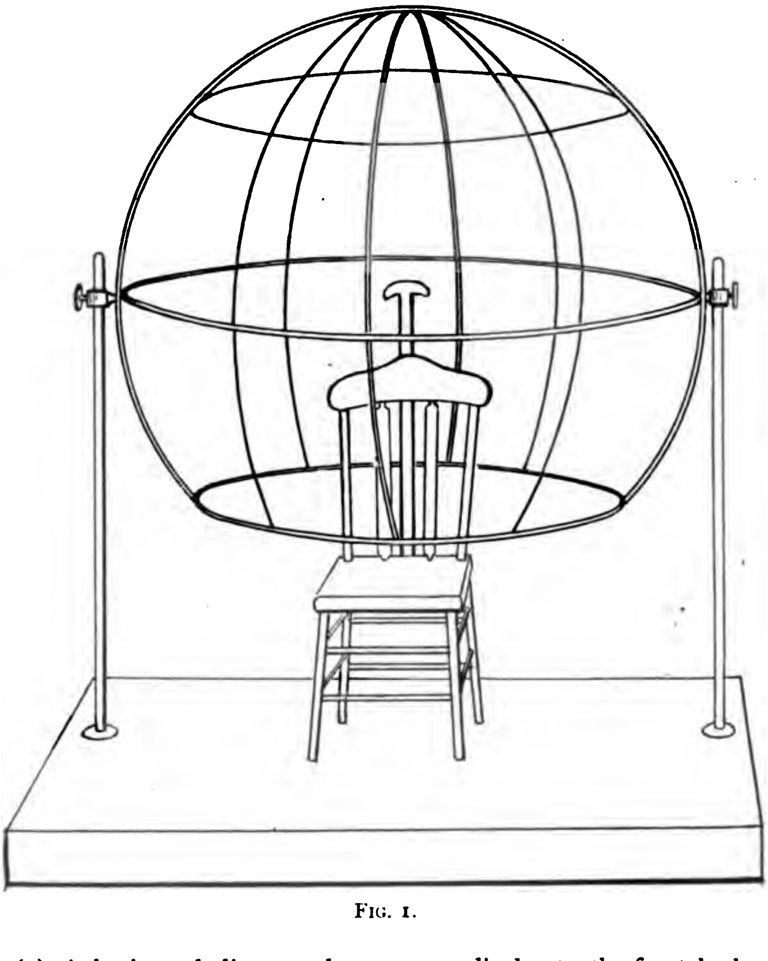

Fig. 1.

(*c*) A horizontal diameter drawn perpendicular to the frontal plane, through the intersecting point of the frontal and the vertical axes. This may be called the sagittal or the *fb* (front-back) axis.

The plane determined by the sagittal and the vertical axes is called the the sagittal or median plane, while the plane determined by the sagittal

and the frontal axes is called the horizontal plane. In the present essay no use will be made of median, frontal or horizontal planes except the primary ones, as just defined; the terms are always to be understood in this way.

The above three axes correspond to the *X*, *Y*, *Z* axes of the Cartesian system of coördinates, and represent the fundamental axes upon which our standard space is constructed with ourselves as the center.

II. Six secondary axes, every two of which lie in the plane determined by the primary axes and cut each other at right angles.

(*d*) Two secondary frontal axes. These are the diameters lying in the frontal plane at the distance of 45° from the frontal and from the vertical axis.

(*e*) Two secondary sagittal axes. These are the diameters lying in the median plane at the distance of 45° from the sagittal and from the vertical axis.

(*f*) Two secondary horizontal axes. These are the diameters lying in the horizontal plane at the distance of 45° from the sagittal and from the frontal axis.

III. Four tertiary axes. These are the diameters lying at 45° from the three neighboring secondary axes in each case.

These thirteen axes are illustrated in the model, Figure 2.

The 26 terminal points of the 13 axes are named in the following way:

I. The 6 terminal points of the 3 primary axes are *f* (front), *b* (back), *o* (over), *u* (under), *r* (right), *l* (left).

II. The 12 terminal points of the 6 secondary axes are:

(*a*) *fo* (front-over), *bu* (back-under), *fu* (front-under), *bo* (back-over).

(*b*) *or* (over-right), *ul* (under-left), *ur* (under-right), *ol* (over-left).

(*c*) *fr* (front-right), *bl* (back-left), *fl* (front-left), *br* (back-right).

III. The 8 terminal points of the 4 tertiary axes are:

(*a*) *for* (front-over-right), *bul* (back-under-left).

(*b*) *fol* (front-over-left), *bur* (back-under-right).

(*c*) *bor* (back-over-right), *ful* (front-under-left).

(*d*) *bol* (back-over-left), *fur* (front-under-right).

The person to be experimented upon is seated in the inside of the cage; his head is adjusted by means of a head-rest fixed to the back of the chair in such a way that his visual axis in the normal position of the body will lie in the median plane, and his auditory axis (an imaginary line drawn through the openings of the ears) with the frontal axis. Then the experimenter gives a short sound at one of the 26 terminal points, and the observer, with his eyes closed, is to judge the direction of the sound. In

my experiments the sound was produced by means of a telephone or a small metallic hammer. Fifty experiments were made for each of the 26 points. The observer was Mr. T. Oku, a student of philosophy.

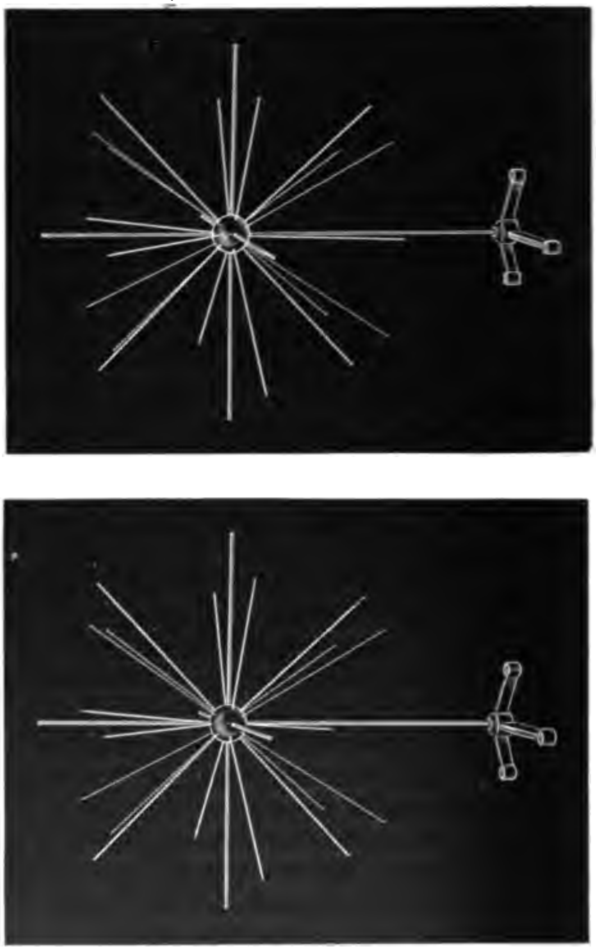

Fig. 2.

(When viewed with a stereoscope the model appears in relief.)

If we could not perceive the direction of sound at all, it would be, as PREYER[1] noticed, theoretically possible that each of the 26 directions would be confused with each of the remaining 25 directions so that there would occur in all $26^2 - 26 = 650$ confusions. Therefore in 676 experi-

[1] PREYER, *Die Wahrnehmung der Schallrichtung mittelst der Bogengänge*, Archiv f. d. ges. Physiol. (Pflüger), 1887 XL 586.

ments the correct judgments would amount only to 26. Or in 1,000 experiments the correct judgments would not exceed 40. This was not the case, for in 1,300 experiments (780 telephone sounds and 520 metallic clicks) it was found that the correct judgments amounted to 768, namely, $\frac{3}{5}$ of the total number instead of $\frac{1}{25}$ as theoretically required. Therefore, the perception of the direction of sound cannot be regarded as purely accidental.

It was noticed that in these experiments none of the 26 directions was actually confused with more than 8 directions. Of 650 possible kinds of errors only 113 kinds were actually observed in our 1,300 experiments and, indeed, many of these 113 kinds occurred only once or twice. What are the remaining kinds of errors which did not occur actually, though they were theoretically possible? This question leads to a very important principle in the perception of the direction of sound. In the experiments the following results were noticed.

1. No sound on the right side was perceived as being on the left side, and no sound on the left side was perceived as being on the right side. That is, none of the series, *r*, *fr*, *for*, *or*, *bor*, *br*, *bur*, *ur*, *fur*, was confused with any of *l*, *fl*, *fol*, *ol*, *bol*, *bl*, *bul*, *ul*, *ful*, and vice versa. As there are 9 *r*-directions and 9 *l*-directions, then 162 (i. e., $2 \times 9 \times 9$) kinds of errors must be subtracted from the total number of errors theoretically possible.

2. No sound on the right or the left side was localized in the median plane. That is, none of the above series was confused with the series in the median plane *f*, *fo*, *o*, *bo*, *b*, *bu*, *u*, *fu*. Therefore we must subtract 144 (i. e., $2 \times 9 \times 8$) kinds of errors from the total number theoretically possible.

3. No sound in the median plane was localized on the right or left side of the plane. That is, none of the series *f*, *fo*, *o*, *bo*, *b*, *bu*, *u*, *fu*, was confused with *l*, *fl*, *fol*, *ol*, *bol*, *bl*, *bul*, *ul*, *ful*, *r*, *fr*, *for*, *or*, *bor*, *br*, *bur*, *ur*, *fur*. Here again 144 (i. e., $2 \times 8 \times 9$) kinds must be subtracted from the total number.

Subtracting these 450 kinds of errors from 650 theoretically possible kinds of errors we get 200 kinds of errors as actually occurring. These 200 kinds of errors are those which will be actually observed. They consist of 72 (i. e., $9 \times 9 - 9$) confusions on the right side, 72 (i. e., $9 \times 9 - 9$) confusions on the left side and 56 (i. e., $8 \times 8 - 8$) confusions in the median plane.

In respect to these three fundamental facts the results of my own experiments perfectly agree with those of PREYER.

These facts lead us to believe that the possession of two ears gives us

an important means of perceiving the direction of a sound. When a sounding body is situated in the median plane, there is no difference between the intensities (and the other possible properties) with which vibratory movements arrive at the two ears. But when a sound is situated outside of the median plane the results will be different and the greater the angular distance from the median plane the greater will be the difference. The relative amount of this difference—the binaural parallax—may give us effective data by which we can judge the direction of the sound. If such a supposition be true, the direction of a sound will be best perceived when it is situated in or around the frontal or auditory axis, for here the difference will be greatest; we can also expect that the direction of a sound will be fairly well recognized when it is situated in the sagittal axis, for that axis is unique in its relation to the two ears. Moreover, it may be expected that the direction of a sound in the horizontal plane will be best perceived, for the shape of the pinna is most favorable for receiving a sound in the horizontal plane, especially in the case of binaural perception. Now let us examine the results of our experiments more closely to see whether they support these suppositions.

For each primary axis the ratio of the correct judgments to the total number was as follows: for *rl* axis, $\frac{87}{100}$; for *fb* axis, $\frac{72}{100}$; for *ou* axis, $\frac{68}{100}$. These results show that the sounds in the *rl* axis are best localized, while the sounds in *fb* axis are better localized than those in *ou* axis. In PREYER's experiments the ratio of the correct judgments for *ou* axis was greater than that for *fb* axis.

Again the ratio of the correct judgments for the 8 directions in each primary plane was as follows: for the horizontal plane (*f*, *fr*, *r*, *br*, *b*, *bl*, *l*, *fl*), $\frac{291}{400}$; for the frontal plane (*o*, *or*, *r*, *ur*, *u*, *ul*, *l*, *ol*), $\frac{271}{400}$; for the median plane (*f*, *fo*, *o*, *bo*, *b*, *bu*, *u*, *fu*), $\frac{288}{400}$. The results show that the sounds in the horizontal plane are localized best of all, while the sounds in the frontal plane are better localized than those in the median plane, these results agree with those of PREYER and ARNHEIM.[1]

The influence of the sounds from the right and left sides is so strong that even the ratio of all correct judgments for those 18 directions in which *r* and *l* take part has a greater value than the ratio of all correct judgments for either of the 18 directions in which *f* and *b* or *o* and *u* take part. In the last two groups *r* and *l* do not occur so often as in the first. The ratios of the correct judgments were as follows: for *rl*

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[1] ARNHEIM, *Beiträge zur Theorie von Schallempfindungen mittelst der Bogengänge*, Diss., Jena 1887.

18 directions, $\frac{530}{900}$; for *fb* 18 directions, $\frac{487}{900}$; for *ou* 18 directions, $\frac{477}{900}$. Our supposition that the possession of two ears gives us by binaural parallax an important means for perceiving the direction of sound seems to be supported by these results. But this general principle is made more complex by various circumstances. For upon examining the number of the correct judgments in the three sets of hemispheres it was found that:

1. The direction of a sound in the right hemisphere was more correctly judged than that of a sound in the left hemisphere. The correct judgments for *r*, *fr*, *for*, *or*, *bor*, *br*, *bur*, *ur*, *fur* amounted to 278, while the correct judgments for *l*, *fl*, *fol*, *ol*, *bol*, *bl*, *bul*, *ul*, *ful* amounted to 257. If the binaural parallax is an important means of localizing a sound, then it is highly probable that the localization will be more or less influenced by the difference in sensitiveness which exists between the two ears. Although the subject was not examined in this respect, it is probable that there was such a difference.

2. The direction of a sound in the front hemisphere was more correctly judged than that of a sound in the rear hemisphere. The correct judgments for *f*, *fo*, *fu*, *fl*, *fol*, *ful*, *fr*, *for*, *fur* amounted to 260, while those for *bo*, *b*, *bu*, *bol*, *bul*, *bl*, *bor*, *br*, *bur* amounted to 237. This difference probably finds its explanation in the function of the pinnæ whose shape is not favorable for receiving sounds in the rear hemisphere.

3. Lastly, a sound in the lower hemisphere was better localized than a sound in the upper hemisphere. The correct judgments for *fu*, *u*, *bu*, *bul*, *ul*, *ful*, *bur*, *ur*, *fur* amounted to 271, while those for *fo*, *o*, *bo*, *bol*, *ol*, *fol*, *bor*, *or*, *for* amounted to 206. We cannot say that this will always be the case, for in PREYER's experiments the sounds were better localized in the upper hemisphere than in the lower hemisphere. The results of my experiments might have been more or less influenced by the probable reflection of sound from the lower parts of the apparatus and the floor, in consequence of which the sound might have been peculiarly colored, as it were, according to its position, and better discriminated by the observer. Apart from such influences the results might have been influenced by the form of the pinnæ.

The examination of these three groups of results enables us to say with great probability that the differences in the degrees of sensitiveness in the two ears and the action of the pinnæ are the factors which render our perception of the direction of sound more or less complex.

Table I. summarizes the results of these preliminary experiments. In this table the objective positions of sounds are indicated in the vertical column at the left, and the perceived directions are given in the hori-

TABLE I.

Position of sound.	*f*	*fo*	*o*	*bo*	*b*	*bu*	*u*	*fu*	*fl*	*fol*	*ol*	*bol*	*bl*	*bul*	*ul*	*ful*	*l*	*fr*	*for*	*or*	*bor*	*br*	*bur*	*ur*	*fur*	*r*	No. of experiments.
	DIRECTION IN WHICH THE SOUND IS PERCEIVED TO LIE.																										
f	**40**	3						7																			50
fo	12	**27**	6	3	1			1																			50
o		11	**32**	7																							50
bo			16	**16**	14	4																					50
b	1			4	**32**	12	1																				50
bu					4	**29**	17																				50
u			1		7	10	**30**	2																			50
fu	6	3	2	1	10	1		**27**																			50
fl									**35**	5	1				2	4	3										50
fol									9	**25**	13					2	1										50
ol										4	**30**	4			1		11										50
bol									3	3	18	**12**	7	1			6										50
bl											2	6	**26**	8	3		5										50
bul													1	**29**	19		1										50
ul									1		1		2	10	**34**		2										50
ful									9		5	2	2		5	**24**	3										50
l									1	1	3				3		**42**										50
fr																		**31**	5	1		4			3	6	50
for																		8	**20**	16	2					4	50
or																			1	**31**	2			2		14	50
bor																			3	13	**13**	8	2	2	3	6	50
br																				1		**40**	6	1	1	1	50
bur																					1	1	**40**	8			50
ur																				1		4	12	**27**	1	5	50
fur																		1	5		4	2	2	4	**31**	1	50
r																				2		1		2		**45**	50
Total	59	44	57	31	68	56	48	37	58	38	73	24	38	48	67	30	74	40	34	65	22	60	62	46	39	82	**1800**

zontal columns. The number of judgments of each kind is given by the figures which are found below each perceived direction.

In the same manner as the actual possibility of the confusion of the directions of sounds is limited to certain distinct regions, so the directions which are *most liable* to be confused with each other are also restricted to narrower limits than the regions of actual possibility.

In the following lists the frequency of the confusion between pairs of the 26 directions is shown by the figures. Here it is regarded as a matter of indifference whether—for example—*fr* is confused with *for*, or *for* with *fr*, and likewise for any other pairs of directions that may be confused.

LIST I.

Confusions in the Median Plane.

Confusion between *f* and *fo* (15), *fu* (13), *b* (1).
" " *fo* and *o* (17), *fu* (4), *bo* (3); *b* (1)
" " *o* and *bo* (23), *fu* (2), *u* (1).
" " *bo* and *b* (18), *bu* (4), *fu* (1).
" " *b* and *bu* (16), *fu* (10), *u* (8).
" " *bu* and *u* (27), *fu* (1).
" " *u* and *fu* (2).

LIST II.

Confusions in the Left Hemisphere.

Confusion between *l* and *ol*(14), *bol* (6), *ul*(5), *bl*(5), *fl*(4), *ful*(3), *fol*(2), *bul*(1).
" " *fl* and *fol*(14), *ful*(13), *bol*(3), *ul*(3), *ol*(1).
" " *fol* and *ol*(17), *bol*(3), *ful* (2).
" " *ol* and *bol*(22), *ful*(5), *ul* (2), *bl*(2).
" " *bol* and *bl*(13), *ful*(2), *bul*(1).
" " *bl* and *bul*(9), *ul* (5), *ful*(2).
" " *bul* and *ul*(29).
" " *ul* and *ful*(5).

LIST 3.

Confusions in the Right Hemisphere.

Confusion between *r* and *or*(16), *ur* (7), *bor*(6), *fr*(6), *for*(4), *br*(2), *fur*(1).
" " *fr* and *for*(13), *fur* (4), *br*(4), *or*(1).
" " *for* and *or*(17), *bor* (5), *fur*(5).
" " *or* and *bor*(15), *ur* (3), *br* (1).
" " *bor* and *br*(8), *fur* (7), *bur*(3), *ur*(2).
" " *br* and *bur*(7), *ur* (5), *fur*(3).
" " *bur* and *ur*(20), *fur* (2).
" " *ur* and *fur*(5).

These results show that the points which are most liable to be confused with each other are those which are situated nearest to each other in the region of actual possibility of confusion. This is in accordance with our view

that the localization depends upon the binaural parallax, for the sounds at the points situated nearest to each other in the regions of actual possibility of confusion are those which are more nearly equal in their relations to the two ears than the sounds at other points. Moreover, we can notice a certain similarity in the three lists above. The errors that occur most frequently in the second list (first two columns) are of similar kinds with the most frequent errors (first two columns) in the third list. Moreover, if we disregard the question of right and left—thereby cutting off *l* and *r* from the members in the second and third lists, then the most frequent errors (first column) will be the same in all three lists. In other words, the errors which occur most frequently in the median plane are repeated with almost the same regularity in the left and right hemispheres. The results are arranged in Table II.

In connection with the above three lists it is interesting to know the relation of the angular magnitude of the error to its frequency. In the median plane the *possible* frequencies of the errors out of a total of 64 are as follows: for 180°, 8 times, or $12\frac{1}{2}$%; for 135°, 16 times, or 25%; for 90°, 16 times, or 25%; for 45°, 16 times, or 25%; for 0°, 8 times, or $12\frac{1}{2}$%.

In the actual experiments, however, 400 judgments were distributed as follows: for 180°, 3 times, or $\frac{7}{10}$%; for 135°, 13 times, or 3%; for 90°, 20 times, or 5%; for 45°, 131 times, or 33%; for 0°, 233 times, or 58%.

Comparing these two series it becomes evident that in reality the errors are not evenly distributed. The error of 0° magnitude (i. e., correct judgment) is by far the most frequent; the error of 45° (next to the smallest magnitude) comes next to it, and the errors of greater magnitudes occur less frequently as the magnitude increases.

The same observation is to be made concerning the errors in separate hemispheres. If the errors were evenly distributed the frequencies for the right and left hemispheres would be as follows: for 119°, 2 × 4 times, or 5% of the total number; for 98°, 2 × 8 times, or 10%; for 90°, 2 × 12 times, or 15%; for 85°, 2 × 8 times, or 10%; for 60°, 2 × 8 times, or 10%; for 59°, 2 × 8 times, or 10%; for 45°, 2 × 24 times, or 30%; for 0°, 2 × 9 times, or 11%.

In the actual experiments, however, 900 judgments were distributed as follows: for 119°, 9 times, or 1% of the total number; for 98°, 7 times, or $\frac{8}{10}$%; for 90°, 30 times, or $3\frac{3}{10}$%; for 85°, 8 times, or $\frac{9}{10}$%; for 60°, 18 times, or 2%; for 59°, 23 times, or $2\frac{5}{10}$%; for 45°, 270 times, or 30%; for 0°, 535 times, or 59%.

Just as in the median plane, the errors of smaller magnitndes happen

TABLE II.

Position of sound.	*f*	*fo*	*o*	*bo*	*b*	*bu*	*u*	*fu*	*l*	*fl*	*fol*	*ol*	*bol*	*bl*	*bul*	*ul*	*ful*	*r*	*fr*	*for*	*or*	*bor*	*br*	*bur*	*ur*	*fur*
f	**40**																									
fo	15	**27**																								
o		17	**32**																							
bo		3	23	**16**																						
b	1	1		18	**32**																					
bu				4	16	**29**																				
u			1		8	27	**30**																			
fu	13	4	2	1	10	1	2	**27**																		
l									**42**																	
fl									4	**35**																
fol									2	14	**25**															
ol									14	1	17	**30**														
bol									6	3	3	22	**12**													
bl									5			2	13	**26**												
bul									1				1	9	**29**											
ul									5	3		2		5	29	**34**										
ful									3	13	2	5	2	2		5	**24**									
r																		**45**								
fr																		6	**31**							
for																		4	13	**20**						
or																		16	1	17	**31**					
bor																		6		5	15	**13**				
br																		2	4		1	8	**40**			
bur																						3	7	**40**		
ur																		7			3	2	5	20	**27**	
fur																		1	4	5		7	3	2	5	**31**

also in the both hemispheres more frequently than the errors of greater magnitudes. The exceptionally great percentage for 90 degrees arises from the familiar confusions between front and back and between above and below ; these will be considered in detail later.

From these results it follows that the smaller the angular distance between the two points, the greater is their confusion with each other. Though this fact is a matter of common experience, the experimental determination of it is very important.

The frequencies of the errors relating to the magnitude, which we actually observed in our experiments, are shown in Table III.

TABLE III.

	0°	45°	59	60°	85	90°	98	119°	135°	180°	Number of experiments.
l	42	7	1								50
r	45	5									50
bl	26	19		5							50
br	40	7		2	1						50
ol	30	19				1					50
or	31	17				2					50
fl	35	12		3							50
fr	31	14		1		4					50
ul	34	12		3		1					50
ur	27	18		4		1					50
bol	12	25	6		3	4					50
bor	13	21	6			5	2	3			50
fol	25	22	1			2					50
for	20	24	4			2					50
bul	29	20	1								50
bur	40	9				1					50
ful	24	14	3		2		5	2			50
fur	31	5	1		2	7		4			50
bo	16	30				4					50
o	32	18									50
fo	27	18				4			1		50
f	40	10									50
u	30	12				7				1	50
b	32	16				1				1	50
bu	29	21									50
fu	27	6				4			12	1	50
Total	768	401	23	18	8	50	7	9	13	3	1300

The foregoing preliminary experiments have shown that the difference between the sensations with which a sound is heard in the two ears must be regarded as the fundamental datum for localizing the sound.

The next step must therefore be a closer examination of this datum of

localization. There are four characteristics of sound-waves by which one sound may be discriminated from another, namely, intensity, pitch, phase and complexity (or timber). The localization of a sound must be based upon a difference in one or more of these four characteristics.

Of these four characteristics it was the question of intensity to which my chief attention was paid in the further experimental work, for from the nature of the subject the problem could be more definitely studied in reference to this characteristic than to the other ones.

II. Dependence of the localization of a perceived sound upon the relative intensities of the sounds heard by the two ears.

We have seen in our preliminary experiments that a sound in the median plane is never localized on the right or the left side and a sound on the right or the left side is never localized in the median plane, and we have assumed that these facts depend upon the peculiar relation between the intensities with which the ears are excited by a sound in the median plane. Now the queston arises whether we do always localize the perceived sound in the median plane when both ears are excited with the same intensity. The following experiments were conducted to get an answer to this question.

1. *Dependence of the localization of a sound in the median plane upon the equal intensities of the impressions in the two ears.*

Each of the primary circles of the spherical cage was divided into degrees. In the horizontal circle the front (*f*) was taken as 0° and the degrees were counted on both sides of the circle from front to back, the back being 180°. In the frontal circle the top was taken as 0° and the degrees were counted from the top downward, the point opposite to the top being 180° and that horizontally either right or left 90°. In the median circle the top was taken as 0°, and the degrees were counted from the top downward, either front or back being 90° and the point opposite to the top 180°.

Two telephones were placed at two symmetrical points of the same circle. The head of the observer was adjusted as in the preliminary experiments. The two telephones were sounded with equal intensities for two seconds. The observer was to judge the direction of the sound. The points at which the telephones were placed are given in Table IV.

For each pair of positions 4 to 8 experiments were made; the total number of experiments was 125. To eliminate the effect of suggestion

and practice, the experiments were made in an irregular order, and not in the order given in the table. Mr. T. Nakashima, a well trained observer, was the subject of the experiments.

TABLE IV.

	A, horizontal.		*B*, frontal.		*C*, median.	
	right	left	right	left	front	back
1	22.5°	22.5°	22.5°	22.5°	22.5°	22.5°
2	45°	45°	45°	45°	45°	45°
3	67°	67°	67°	67°	67°	67°
4	90°	90°	90°	90°	90°	90°
5	112.5°	112.5°	112.5°	112.5°	112.5°	112.5°
6	135°	135°	135°	135°	135°	135°
7	157.5°	157.5°	157.5°	157.5°	157.5°	157.5°
8	0°	180°	0°	180°	0°	180°

The fundamental phenomenon always observed in experiments of this kind is that the two similar impressions received by the two ears were combined into one sound.

The results of these experiments are given in Table V. The table is to be interpreted in the following manner. When the telephones were in the positions given in Table IV, the sound appeared to be in the directions given under similar headings in Table V; thus *A* 1 of the latter corresponds to *A* 1 of the former, etc. The expressions contained in the parentheses represents judgments of this character: "front but a trifle upward," etc. The letter *k* means "in the head;" the other letters have the meanings given on p. 3.

TABLE V.

	A	*B*
1	*j, f(o), f(o), f(ol), b(ol)*	*k, k(b), k(bu), k(o), k(fo), k(bo)*
2	*f, f(u), f(o), b, b(k), k(f), k(f)*	*k, f(o), k(l), k(b), k(b)*
3	*b, b, b(k), b(o), k(r)*	*k, k, k(u), f(o), f(o)*
4	*f, f, b, b(k), b(k), b(o), k, k(b)*	*f, f, b, b(k), b(k), b(o), k, k(b)*
5	*b, b, b(k), b(ko)*	*b, b, k, k(b), bu*
6	*b, b, b, b, b(k)*	*b(u), b(k), b(u), b(ul), b*
7	*b, b, b, b, b(k), b(ko)*	*u(rb), u(rb), u(f), u(r), u(rb)*
8	*f, f, f, f(o), b(k)*	*o, o(b), o(f)*

	C
1	*f(o), f(o), f(o), f(o), f(o)*
2	*f(o), f(o), f(o), f, f*
3	*f(o), f(o), f, f, f*
4	*f, f, f, f(o), b(k)*
5	*b, b(u), f, f, f, f*
6	*b(u), b(u), b, b, b*
7	*u(b), u(b), u(b), b* or *f, f* or *b*
8	*o(f), o(b)*

Table V. shows that all the sounds were localized in the median plane. A slight deflection from the median plane, which is indicated by the letters in parentheses, seems to be the effect of slight deviations in the position of the head, in manner of placing the telephones and in occasional difference between the intensities of the two sounds, all of which we could not govern accurately.

In group *A* most sounds were perceived to be at *b* or *b* (*k*). But when the two ears were stimulated by sounds coming from 22.5° and 22.5°, or 45° and 45°, or 0° and 180°, most of the perceived sounds were perceived to be at *f*.

In group *B* the sound was perceived to be at *k* (in the head) when the two ears were stimulated by sounds from above, whereas it was perceived to be at *b* or *u* when the two ears were stimulated by sounds from below. When the sounds were given at *o* and *u* the sound was perceived to be at *o*.

In group *C* most of the perceived sounds were localized at *fo* and *f* when the two ears were stimulated by sounds situated between 0° and 112.5°. But the sounds were mostly perceived at *b* or *u* when the two ears were stimulated by sounds situated lower than 135°.

The conclusion seems to be justified by the results of this set of experiments that, in whichever of the three primary planes the objective sounds may be placed, the perceived sound is always localized (in so far as the sensitiveness of the two ears is the same and the two objective sounds are exactly equal) in the median plane if these sounds are placed in such a way that the distances between one ear and the sources of the sounds are equal to the distances between the other ear and the same sources respectively.

Since equal distances have equal influences upon the intensity of a sound, the above conclusion can be expressed in terms of intensity, namely, when the two ears are stimulated simultaneously by sounds of equal intensity the perceived sound is always localized in the median plane. Conversely we can say that when a sound is localized in the median plane the intensities of the impressions in the two ears are equal.

There still remains the question concerning the component upon which depends the discrimination between front and back, above and below in the median plane. It is true that in the median plane the localization is very imperfect. Still the existence of some localizing power in this plane was proved by the results of the experiments conducted by v. KRIES.[1] Here the localization cannot be explained by the principle of relative

[1] v. KRIES, *Ueber das Erkennen der Schallrichtung*, Zt. f. Psych. u. Physiol. d. Sinn., 1890 I 235.

intensity, for the two ears are stimulated with the same intensity. The experiments of v. KRIES and RAYLEIGH [1] show us that the possibility of the localization in the median plane depends to a great extent upon the constitution of the sound and upon practice. A pure tone, such as that produced by a tuning fork, is in general localized distinctly only with difficulty, but a noise or a tone mixed with overtones (such as the noise produced by striking small blocks against each other, or the human voice) seems to be better localized. The difference which exists between the two cases seems to arise—though it it is not easy to make it definite by an experiment—from the fact that the quality and pitch of a sound are more or less modified according to its position in the median plane, for the sound waves will be more or less influenced by the position of the sound with respect to the position of the pinnæ. Not only the quality and the pitch, but also the absolute intensity of the sound will be different according to its position. These factors will be considered later.

2. *Dependence of the localization of a sound in the horizontal plane upon the unequal intensities of the impressions in the two ears.*

When a source of sound is situated not far from us, either on the left or on the right side, the intensities of the impressions produced in the two ears are not equal, for the intensity of a sound varies (according to the generally accepted law of propagation of sounds) inversely as the square of the distance. The difference between the distances becomes smaller as the source of sound approaches the median plane, while it grows greater as the source of sound moves more toward the side.

The question is whether or not we localize the perceived sound at different points according to the change in the relative difference between the intensities of the sensations received in the two ears. A definite answer was sought by the following set of experiments.

In the preceding experiments I noticed that the reflection of the sound from the surrounding walls had some influence upon the localization. It seemed desirable in further experiments to avoid this source of error, as far as possible. A small separate chamber 4 feet long, 4 feet wide and 4 feet high, with walls of felt, was arranged in a quiet spacious room on the top floor of the Yale Laboratory. Instead of a spherical cage as used in the foregoing experiments the following arrangement was made for determining the objective positions of the telephones.

On the floor of the chamber a circle was described, whose radius was 65^{cm}. This circle was divided into 12 equal parts by 12 radii at 30°

[1] RAYLEIGH, *Our perception of the direction of a source of sound*, Nature, 1876 XIV 32.

apart. The person experimented upon was seated in the center of the chamber. His head was adjusted by a support in such a way that the line connecting the openings of the two ears would intersect at its middle point an imaginary line drawn from the center of the circle perpendicularly to the floor. In order to eliminate the influence of suggestion upon judgment, the eyes of the observer were blindfolded before he was allowed to enter the chamber. He consequently never knew anything of its construction or contents. Two telephone-stands of a T shape were prepared. Each stand could be erected on any of the 12 divisions in such away that the longer arm would be perpendicular to the floor and the shorter one would be parallel to the radius at that point. A telephone was hung on the shorter arm by means of strings. The height of the shorter arm was adjusted so that the telephone would lie in the same line with the openings of the ears.

The wires from one telephone were connected with the secondary coil of a sliding inductorium. The wires from the other telephone were connected with the two binding posts at one end of the primary coil, where the electric current coming through an electro-magnetic tuning fork in another room was to be divided into two circuits, one of which served for the primary circuit of the inductorium and the other for the circuit of the second telephone. The current for the latter telephone passed through a copper sulphate rheostat. By means of this rheostat the intensity of the current—and consequently the intensity of the telephone sound—could be regulated. The secondary coil, with which the wires of the first telephone were connected, carried a pointer which passed over the divisions of the millimeter-scale on the base of the inductorium. By changing the distance between the coil the intensity of the induced current—and consequently the intensity of the telephone sound—could be regulated.

In this set of experiments a self-interrupting electro-magnetic fork of 250 complete vibrations was placed as a shunt across the telephone circuit. The current from a lamp battery [1] passed through the fork during half its period of vibration, while during the other half of the period it passed to the telephone apparatus. Thus a tone of 250 vibrations could be produced in the telephones.

The standard intensity of the current could be regulated at pleasure by changing the lamps of the battery. When the telephone circuit was closed by the key each telephone produced a sound of definite pitch, with an intensity depending upon the amount of liquid resistance introduced or upon the distance of the secondary coil from the primary one.

[1] SCRIPTURE, *New apparatus and methods*, Stud. Yale Psych. Lab., 1896 IV 76.

First group.

In the first group of experiments the telephones were placed directly opposite the openings of the ears at a distance of 40^{cm}. The current was turned on for a little more than a second, the two sounds being produced simultaneously. When the sounds ceased the observer was to announce the direction of the sound which he perceived. The experimenter was to take the record both of that direction and of the distance of the secondary coil from the primary, which represented the intensity of the current for one telephone. During the experiment the intensity of the current for the other telephone was kept constant.

The first subject was A. Fisher, the laboratory janitor, a well-trained observer without the slightest knowledge of the arrangements or interest in the results. He always perceived only one sound instead of two separate sounds and projected the sound in one of five directions in a horizontal plane about the same level as his eyes, i. e., at *r*, *fr*, *f*, *fl*, *l*. according to the difference between the intensities of the two sounds. If we call for the sake of convenience the relative intensities "strongest," "stronger," "equal," "weaker" and "weakest," the results can be summarized as follows.

1. When *R*, intensity of the component of the sound on the right side, was "strongest," and *L*, intensity of the component of the sound on the left side, was "weakest," the perceived sound was projected toward *r*.

2. When *R* was "stronger" and *L* "weaker" the perceived sound was projected toward *fr*.

3. When *R* and *L* were "equal" the perceived sound was projected toward *f*.

4. When *R* was "weaker" and *L* "stronger" the perceived sound was projected toward *fl*.

5. When *R* was "weakest" and *L* "strongest" the perceived sound was projected toward *l*.

The next subject was Dr. C. E. Seashore, assistant in the laboratory. He knew where the two telephones were placed and how the intensities would be varied. The results were practically the same as those of the first observer. The only difference was that the second observer could distinguish finer differences of direction than the first observer could. This suggested the possibility of having the results stated in a scale of degrees. This was tried with success on Mr. C. Wakamatsu, a young Japanese student of science, who was totally ignorant of the method of the experiment and the arrangement of the apparatus.

Before stating the results it must be made clear that the exact relation between the intensity of the electric current and the intensity of the telephone sound is not known, and that the rate of change in the intensity of the current which corresponds to the distance of the secondary coil from the primary can not be numerically stated. All we can say is that the intensity of the telephone sound increases with the increase of the induced current and that the intensity of the induced current becomes stronger as the secondary coil is moved nearer to the primary coil. The rate of change in the intensity is not constant; in the particular instrument employed it is rather rapid between 2^{cm} and 4^{cm}, slow between 4^{cm} and 9^{cm} and rapid again beyond 9^{cm}. It must also be noted that we cannot average directly the results for several days; owing to the nature of the experiment we can not keep all the conditions perfectly constant during different days. Slight changes in the positions both of the telephones and the head of the observer and minor errors in apparatus were sufficient to produce somewhat varying results. I am, therefore, compelled in all succeeding experiments to take the average of the results of experiments conducted within a few hours on the same day, during which the above mentioned conditions could be kept tolerably constant. On the present occasion no attempt will be made to establish a numerical relation between the variation in the relative intensities of the two sounds and the variation in the localization of the perceived sound. We must be satisfied if the general dependence of the latter upon the former is proven.

TABLE VI.

Case I.			Case II.		
Distance of the secondary coil for the left telephone.	Localization.	Number of experiments.	Distance of the secondary coil for the right telephone.	Localization.	Number of experiments.
10^{cm}	*fr* 50°	3	10^{cm}	*fl* 80°	2
9	*fr* 27.5	2	9	*fl* 75	2
8	*fr* 25	2	8	*fl* 60	2
7	*fr* 22.5	2	7	*fl* 41.7	3
6 5	*fr* 18	2	6.5	*bl* 70	2
6	*fr* 10	1	6	*f*	2
5	*fl* 0.5	2	5	*f* or *b*	2
4	*fl* 0.5	2	4	*fr* 60	2
3	*fl* 25	2	3	*fr* 60 or *br* 60	2
2	*fl* 27.5	2	2	*fr* 60	2
1	*fl* 55	2	1	*fr* 60	2
0.5	*fl* 75	1	0.5	*fr* 82.5	2

In Case I the probable error varies from 0 to ± 30%.

In Case II the probable error varies from 0 to ± 8%.

The averages for the experiments are given in Table VI. Case I gives the results when the left sound was varied while the other was kept constant, and Case II gives the results when the right sound was varied and the other was kept constant. In these and subsequent similar experiments, the observer announced the direction of the perceived sound in a scale of degrees, taking *f* and *b* as 0° and counting toward *r* and *l*. For example, *fr* 60° means that the sound is in the front 60° toward *r* and *br* 60° means that the sound is in the rear 60° toward *r*.

To eliminate the effect of suggestion the experiments were made, as in the preceeding section, in an irregular order.

The general relations which were stated on page 18 are shown here more plainly. The transition of the perceived direction according to the change in the relative intensities of the two sounds is not only more gradual, but more minutely scaled. Figure 3 shows diagrammatically the results of the experiments.[1] This figure and the similar figures in the following pages represent the mental field of localization of sound. The median plane of the auditory field coincides with the real median plane of the head, but the mental right and left do not seem to lie exactly in the frontal plane of the head. The directions that appear to us a right and left, seem to lie slightly in front of the auditory axis. The apparent right and left seem to be determined by visual sensations and consequently to lie in a line tangent to both eyes. This seems to afford some explanation of the fact that we localize the preceived sound slightly in the back of the apparent right and left line when the two component sounds have the maximum relative difference in intensity.

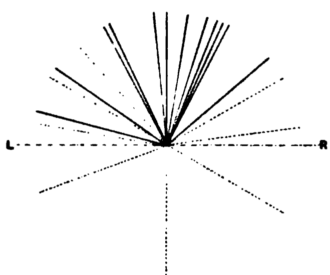

FIG. 3.

In comparing the localizations of Case II with those of Case I we find that the latter is surer and finer than the former. In Case II we find not only a less careful angular scale, but sometimes a bewilderment of judgment as to whether the sound came from the front or from the rear. This difference seems to have its origin in the difference in sensitiveness between the two ears of the observer. The sensitiveness of each ear of the observer was examined by means of the audiometer[2] and it was found

[1] In this and the next figures the full lines represent Case I and the dotted lines Case II.

[2] According to the method described by SCRIPTURE, *Threshold of intensity for sound*, Stud. Yale Psych. Lab., 1896 IV 103.

that the ratio of the sensitiveness of the right ear to that of the left ear was as 10 to 11. It is probable that the discriminating ability for the change in the intensity of a sound depends upon the sensitiveness of the ear. As the right ear of the observer was less sharp than the left ear, the discriminating ability of the former would be less than that of the lat. ter. Accordingly, when the variable soundwas on the left side and the constant sound was on the right side the difference between the intensities of the two sounds would be more accurately perceived than when the sounds were given in the other way.

Moreover, when the variable sound was on the right side and the constant sound on the left side the observer tended sometimes to localize the perceived sound in the rear hemisphere instead of the front, or sometimes he could not decide whether the sound was front or back, though he perceived the angular displacement of the sound from 0°, for example, he could not decide whether the sound came from *f* or *b*, *fr* 60° or *br* 60°. The same uncertainty will be found again in later experiments. The cause of such confusion between front and back location, as we may call it, must be sought in the similarity of the relation between intensities with which the sounds situated in the two directions in question are received by the two ears respectively. This point will receive special consideration later.

TABLE VII.

Case I.			Case II.		
Distance of the secondary coil for the left telephone.	Localization.	Number of experiments.	Distance of the secondary coil for the right telephone.	Localization.	Number of experiments.
12cm	*br* 70°	1	12cm	*l* 90°	1
11	*br* 52.5	2	11		
10	*br* 45	3	10	*bl* 75	3
9	*br* 31	4	9	*bl* 72	3
8	*br* 10	3	8	*bl* 20	3
7	*b*	1	7	*br* 5	3
6.5	*bl* 5	4	6.5	*br* 13	3
6	*bl* 25	4	6	*br* 40	3
5	*bl* 30	3	5	*br* 66.7	3
4	*bl* 57	5	4	*br* 23.3	3
3	*bl* 50	3	3	*br* 83.3	3
2	*l* 90	2	2	*r* 90	3
1	*l* 90	2	1	*r* 90	3

In Case I the probable error varies from 0 to ± 11%.

In Case II the probable error varies from 0 to ± 32%.

We must at any rate conclude that the dependence of the localization upon the relative difference between the intensities of the impressions

in the two ears does not necessitate that the perceived sound will, under the conditions of our experiments, always be localized forward. It is quite reasonable to suppose that the perceived sound will sometimes be localized backward under the same conditions, especially when the intensity of the perceived sound is weakened on account of physical, physiological or mental circumstances. In connection with this the results of experiments upon another person, Mr. W. S. Johnson (a student of psychology), are interesting. He localized all sounds backwards. The average results of the experiments upon him are shown in Table VII. Case I gives the results when the sound of the left telephone was varied, and Case II the results when the sound of the right telephone was varied Figure 4 shows the results diagrammatically.

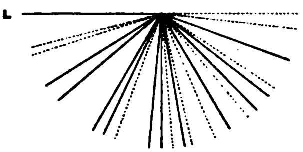

FIG. 4.

The preceding experiments were made without any previous practice, and the observers were required to tell only the direction of the perceived sound. It was found that after some practice an observer could tell not only the direction, but also the distance, of the perceived sound. So the experiments were repeated upon Mr. C. Wakamatsu to determine what would be the apparent distance of the sound. By this time the observer was somewhat practiced in the work, and the judgments were more definitely announced than in the previous experiments. The following table shows the average results. The Japanese unit "sun" was used by the observer in estimating the linear distances. One "sun" is equal to 3.03 cm. nearly.

TABLE VIII.

CASE I.

Distance of the secondary coil for the left telephone.	Judgment of direction.	Judgment of distance.		Number of experiments.
12cm	*fr* 76°	7.3sun	(22cm)	3
11	*fr* 67	8.7	(26)	3
10	*fr* 70	7	(21)	3
9	*fr* 27	10	(30)	3
8	*fl* 12	9.3	(28)	3
7	*fl* 30	8.7	(26)	3
6	*fl* 47	7.7	(23)	3
5	*fl* 70	5	(15)	3
4	*fl* 77	3.7	(11)	3
3	*fl* 80	3.3	(10)	3
2	*fl* 83	3	(9)	3
1	*fl* 83	3	(9)	3

In Case I the probable error for direction varies from 0 to ± 8% and that for distance varies from 0 to ± 3%.

CASE II.

Distance of the secondary coil for the right telephone.	Judgment of direction.	Judgment of distance.		Number of experiments.
12^{cm}	*bl* 85°	8^{cm}	(26^{cm})	2
11	*bl* 82.5	7.7	(23)	3
10	*bl* 55 ; *bl* 30 or *fl* 30	8	(24)	3
9	*fl* 60 ; *bl* 40 ; *fl* 20 or *bl* 20	6 or 8	(18 or 24)	3
8	*b* or *f*	8.7	(26)	3
7	*br* 50	6.3	(19)	3
6	*fr* 65 ; *br* 50	5.5	(17)	3
5	*fr* 85 ; *br* 50	4.5 or 5	(14 or 15)	3
4	*fr* 80	4.5	(14)	3
3	*r* 90	3.2	(10)	3
2	*r* 90	3	(9)	2
1	*r* 90	3	(9)	1

In Case II the probable error for direction varies from 0 to ± 3% and that for distance varies from 0 to ± 11%.

Figure 5 shows diagrammatically the results for Case I and Figure 6 the results for Case II.

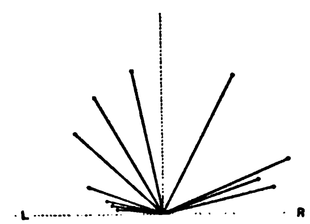

FIG. 5.

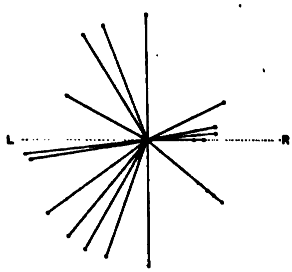

FIG. 6.

The judgment of direction was surer in Case I than in Case II, as in the preceding experiments on the same person. In Case II the sound which was perceived under the same conditions was at one time projected at *f* and another time at *b*, or the sounds which were at one time projected at *fl* 20° and *fl* 30° were at another time projected at *bl* 20° and *bl* 30° respectively.

As to the perception of distance the intensity of the perceived sound seems to furnish the data for the judgments. In the above experiments the intensity of the variable sound was strongest when the secondary coil was at the distance of 1^{cm}; it grew less strong as the distance was greater, and became weakest at 12^{cm}. Accordingly the distance of the sound was perceived as least when the secondary coil was 1^{cm} distant

from the primary, and it was perceived to be greater as the coil was moved towards the other end of the scale. The gradual change in the distance according to the change in the intensity is clearly seen in the results.

The observer seemed to choose a certain intensity as a standard to which he ascribed a certain distance; the distance of another sound seemed to be judged by comparing its intensity with this standard intensity. As a consequence the estimate of distance is different, at least so far as our experiment goes, in different individuals. This can be seen by comparing the above results with the following results which were obtained by making the same experiment on the observer, Mr. W. S. Johnson, in whose case all sounds were, as before, projected towards the back. He estimated the distance in inches (1 inch = $2\frac{1}{2}^{cm}$).

TABLE IX.

CASE I.

Distance of the secondary coil for the left telephone.	Judgment of direction.	Judgment of distance.		Number of experiments.
11^{cm}	*r* 90°	48^{in}	(120^{cm})	2
10	*r* 90	57	(143)	4
9	*br* 52	51	(128)	4
8	*b*	36	(90)	4
7	*bl* 7.5	36	(90)	4
6	*bl* 53	28.5	(71)	4
5	*bl* 75	16.5	(41)	4
4	*l* 90	8	(20)	3
3	*l* 90	6	(15)	1

In Case I the probable error for direction varies from 0 to ± 17% and that for distance varies from 0 to ± 10%.

CASE II.

Distance of the secondary coil for the right telephone.	Judgment of direction.	Judgment of distance.		Number of experiments.
11^{cm}	*l* 90°	60^{in}	(150^{cm})	4
10	*bl* 82.5	57	(143)	4
9	*l* 90	60	(150)	4
8	*b*	39	(98)	4
7	*br* 15	31.5	(79)	4
6	*br* 40	33.3	(83)	4
5	*br* 60	26	(65)	4
4	*br* 82.5	6	(15)	4
3	*r* 90	6	(15)	1

In Case II the probable error for direction varies from 0 to ± 16% and that for distance varies from 0 to ± 9%.

Thus in Johnson's case the greatest distance was 60 inches (150^{cm}), while in the case of Wakamatsu it was about 12 inches (30^{cm}); the shortest distance was 6 inches (15^{cm}) in the case of the former and 3.7 inches (9^{cm}) in the case of the latter.

In the preceding experiments the intensity of the sound was not changed in regular succession, for it was desirable to eliminate the influence of suggestion from the results. It seemed, however, desirable to repeat the experiments by changing the intensity of sound, both descending and ascending in regular order. The average results of the experiments conducted in this way are given in Table X. Here the changes in the direction and the distance were perceived with greater regularity, but in other respects the results were not greatly different from those of the preceding experiments.

Figures 7 and 8 show the results diagrammatically.

In closing the experiments of the first group I tried to see whether a curve of the localized points for the perceived sounds could be obtained if the two sounds were given continuously for a certain length of time during which the intensity of one of the two component sounds was varied in diminuendo or in crescendo. The experiments were made on the observer C. W.

a. When the intensity of the left sound was changed in diminuendo, by sliding the secondary coil from 1^{cm} to 15^{cm}, while keeping the intensity of the right sound constant, the sound was perceived as if travelling

TABLE X.

CASE I.

Distance of the secondary coil for the left telephone.	Judgment of direction.	Judgment of distance.	Number of experiments.
13^{cm}	*r* 90°	8.7^{inn} (26^{cm})	3
12	*fr* 76.7	9 (27)	3
11	*fr* 76.7	9 (27)	3
10	*fr* 73.3	9.3 (28)	3
9	*fr* 61.7	10 (30)	3
8	*fr* 33.3	10 (30)	3
7	*fr* 13.3	9.3 (28)	3
6	*f* or *b*; *fl* 15	8; 9 (24; 27)	3
5	*fl* 26.7	8 (24)	3
4	*fl* 40	6.7 (20)	3
3	*fl* 43.3	5.7 (17)	3
2	*fl* 70	4.5 (13)	3
1	*fl* 70	4.3 (13)	3

In Case I the probable error for direction varies from 0 to ± 25% and that for the distance from 0 to ± 5%.

CASE II.

Distance of the secondary coil for the right telephone.	Judgment of direction.	Judgment of distance.	Number of experiments.
13cm	*bl* 80°	10sun (30cm)	3
12	*bl* 76.7	10 (30)	3
11	*bl* 70; *fl* 85	10 (30)	3
10	*fl* 73.3	10 (30)	3
9	*fl* 43.3	10 (30)	3
8	*fl* 30; *bl* 30	10 (30)	3
7	*fr* 35; *f*	9; 8 (27; 24)	3
6	*fr* 43.3	8 (24)	3
5	*fr* 55; *br* 80	7; 5 (21; 15)	3
4	*br* 80	4 (12)	3
3	*fr* 80; *br* 80	4 (12)	3
2	*r* 90	3.7 (11)	3
1	*br* 83.3	3.7 (11)	3

In Case II the probable error for direction varies from 0 to ± 18% and that for distance from 0 to ± 8%.

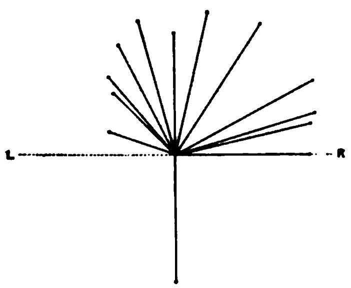

FIG. 7.

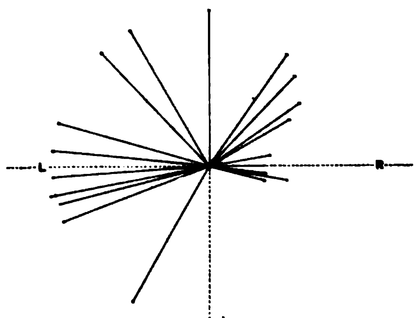

FIG. 8.

from *fl* 80° or *l* 90°, 3sun (9cm), passing to the front at a distance of 6sun (18cm) and stopping at *fr* 80°, or *R* 90°, 12sun (36cm), so that the point of localization described a semi-oval ring with narrow end directed to the right of the observer.

When the intensity of the left sound was varied in crescendo by sliding the secondary coil in the reverse way, the perceived sound started at *fr* 80°, 10sun (30cm), passed to the front at a distance of 7sun (21cm) and stopped at *l* 90°, 4sun (12cm). Sometimes after the sound had reached about *fl* 60° the observer became uncertain whether the sound travelled in front from that point to *l* 90° or travelled in back from *bl* 60° to *l* 90°.

b. When the intensity of the right sound was varied in diminuendo,

and that of the left sound was kept constant, the sound was perceived as if travelling from *r* 90°, 3[sun] (9[cm]), passing to the front and stopping at *l* 90°, 8[sun], 24[cm]. The sound seemed sometimes to travel in the rear, though at some points it was uncertain whether the sound was to the rear or the front.

When the intensity of the right sound was varied in crescendo the sound seemed to start at *bl* 80°, 10[sun] (30[cm]) and travelling to the rear, to stop at *br* 80°, 4[sun] (12[cm]).

This completes the record of the first group. The results show conclusively the existence of a continuous functional relation between the relative difference in intensity (between the impressions in the two ears) and the localization in direction.

Moreover, it can be considered as established that a perceived sound is located on the side from which the stronger sensation is received, the greater the relative difference between the two sensations, the greater being the angular magnitude of the side-localization.

Second group.

When we perceive a sound as situated in the horizontal plane, the intensities of the sensations in the two ears are always different, except when the source of sound is situated in the sagittal, or *fb*, axis. The difference between the two intensities is greatest when the sound is situated nearly in the auditory, or *rl*, axis, for here the difference of distances between the source of the sound and the two ears is greatest.[1] When the source of sound moves gradually from the auditory axis and at the same time approaches the sagittal axis this difference becomes smaller. It has also been shown in the first group of experiments that the localization depends upon the value of the difference between the intensities of the sensations in the two ears. From these two facts it is to be expected that when each ear is affected by two sounds coming from symmetrical points on the two sides respectively the limits within which the perceived sound is localized will be different according to the positions of the symmetrical points.

We have already studied the field of localization of the perceived

[1] It must be noticed here that even when the two ears are equally sharp the difference between the intensities of the sounds heard by the two ears cannot be determined simply by taking the inverse ratio between the squares of the distances from the sound to the ears. When an exact expression is wanted, we must take into account the effects of the refraction of the sound-waves around the head, the reflection of the sound from the surrounding walls, and of the conduction of the sound from one tympanum to the other through the head.

sound when two objective components are placed at *r* and *l*, that is, when the relative difference in intensity between the impressions in the two ears is greatest. In that case the field of localization covered almost an entire semicircle either in front or in back, and sometimes covered more than an entire semicircle. Our next task is to inquire whether this field of localization would be contracted if the relative differences were made smaller. To answer the question, experiments were made on the observer C. W. under the following conditions:

1. Two telephones were situated at *fr* 60° and *fl* 60° respectively.
2. Two telephones were situated at *fr* 30° and *fl* 30° respectively.
3. Two telephones were situated at *br* 60° and *bl* 60° respectively.
4. Two telephones were situated at *br* 30° and *bl* 30° respectively.

The average results of the experiments under condition 1. are shown in Table XI.

TABLE XI.

Case I.				Case II.			
Distance of secondary coil for left telephone.	Judgment of direction.	Judgment of distance.		Distance of secondary coil for right telephone.	Judgment of direction.	Judgment of distance.	
12cm	*fr* 46.7°	10.3sun	(31cm)	12cm	*fl* 36.6°	11sun	(33cm)
11	*fr* 40.8	10.	(30)	11	*fl* 31.7	10.7	(31)
10	*fr* 18.3	11.1	(33)	10	*fl* 23.3	11	(33)
9	*fr* 10	11	(33)	9	*f* 0	11	(33)
8	*fl* 21.7	9.1	(27)	8	*fr* 15.7	9.9	(30)
7	*fl* 46.7	8.4	(25)	7	*fr* 38.3	8.4	(25)
6	*fl* 50.8	6.9	(21)	6	*fr* 48.3	6.9	(21)
5	*fl* 56.6	5.9	(18)	5	*fr* 45	6.3	(19)
4	*fl* 60	4.9	(15)	4	*fr* 48.3	6.4	(19)
3	*fl* 60	4.3	(13)	3	*fr* 48.3	4.6	(14)
2	*fl* 62	3.9	(12)	2	*fr* 51.7	4.3	(13)
1	*fl* 60	3.5	(11)	1	*fr* 48.3	3.8	(11)

The number of experiments on each point is 6.

In Case I the probable error for direction varies from 0 to ± 57%, and that for distance from ± 3% to ± 13%.

In Case II the probable error for direction varies from ± 3% to ± 17% and that for distance from $\frac{8}{10}$% to 8%.

The results are graphically represented in Figures 9 and 10.

Here the relative difference between the intensities of the sensations in the two ears must, for external reasons, have been smaller than that in the preceding experiments. The field of localization of the perceived sound was accordingly much more contracted. When the sound of the left telephone was varied, the field covered a sector included between *fl* 62° and *fr* 46.7°. When the sound of the right telephone was varied, the field was still more contracted, covering a sector included between *fl* 36.6°

and *fr* 51.7°. In both cases most of the perceived sounds were projected on the side on which the source of the variable component sound was situated. Again, in both cases no perceived sound was projected back-

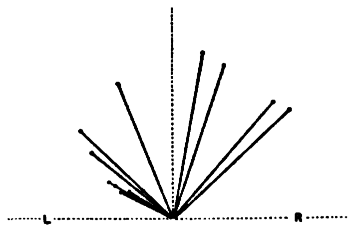

FIG. 9.

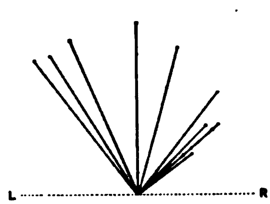

FIG. 10.

ward, and no doubt existed as to the position of the perceived sound, as was the case when two telephones were situated at right and left. The average results of the experiments under condition 2. are given in Table XII.

TABLE XII.

CASE I.			CASE II.		
Distance of the secondary coil for the left telephone.	Judgment of direction.	Judgment of distance.	Distance of the secondary coil for the right telephone.	Judgment of direction.	Judgment of distance.
12^{cm}	*fr* 36.7°	11.3^{mm} (34^{cm})	12^{cm}	*fl* 50°	9^{mm} (27^{cm})
11	*fr* 35	11.3 (34)	11	*fl* 46.7	10 (30)
10	*fr* 30	11.3 (34)	10	*fl* 33.3	9.3 (28)
9	*fr* 26.7	11 (33)	9	*fl* 36.7	10 (30)
8	*fr* 20	11.3 (34)	8	*fl* 23.3	9.3 (28)
7	*fr* 7	10 (30)	7.5	*f* 0	10 (30)
6.8	*f* 0	10 (30)	7	*fr* 3.3	9.5 (28)
6	*fl* 16.7	9.3 (28)	6	*fr* 20	9.3 (28)
5	*fl* 16.7	9 (27)	5	*fr* 36.7	8.3 (25)
4	*fl* 30	6.5 (19)	4	*fr* 40	6.3 (19)
3	*fl* 36.7	5.7 (17)	3	*fr* 33.3	6 (18)
2	*fl* 40	5 (15)	2	*fr* 40	4.3 (13)
1	*fl* 40	4.5 (13)	1	*fr* 36.6	4.6 (14)

The number of experiments on each point is 3.

In Case I the probable error for direction varies from 0 to ± 31% and that for distance from 0 to 10%.

In Case II the probable error for direction varies from 0 to ± 19% and that for distance from 0 to ± 10%.

Figures 11 and 12 represent these results graphically.

In Case I the field of localization covered a sector included between *fr* 36.7° and *fl* 40° and in Case II it covered a sector included between

fl 50° and *fr* 40°. The observer was sure of his judgments and never projected the sound backward. The tendency to project the perceived sound more on the side on which the source of the variable sound was situated was lessened here, and the localizations were more evenly distributed.

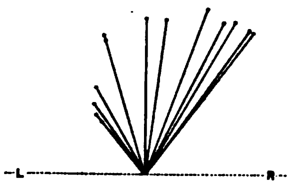

FIG. 11.

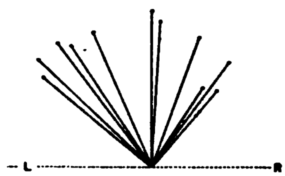

FIG. 12.

A similar contraction of the field of localization of the perceived sound was observed with the same subject when the sources of sound were situated behind the observer. Here we noticed that the perceived sounds were frequently located in front. Under condition 3., when the intensity of the sound on the side of the sharper ear (left) was varied, most of the perceived sounds were located in front, 62 of 72 perceived sounds being decidedly located in front. On the contrary, when the intensity of the sound on the side of the duller ear was varied most of the perceived sounds were located in back, though some of them were located in front. Table XIII gives the average results for the experiments.

TABLE XIII.

CASE I.

Distance of the secondary coil for the left telephone.	Usual localization.		Occasional localization.	
	Direction.	Distance.	Direction.	Distance.
12^{cm}	*fr* 64°	10^{mm} (30^{cm})	*br* 70°	6.5^{mm} (19^{cm})
11	*fr* 56	9.4 (28)	*br* 75	7 (21)
10	*fr* 44	10 (30)	*br* 70	6.5 (19)
9	*fr* 40	9.6 (29)	*br* 30	7 (21)
8	*fl* 8.5	10 (30)	*br* 20	6.5 (19)
7	*fl* 31	9 (27)		
6	*fl* 51.6	6.9 (21)		
5	*fl* 60	6.4 (19)	*bl* 40	5 (15)
4	*fl* 66.7	4.5 (14)		
3	*fl* 70	3.8 (11)		
2	*fl* 72	3 (9)	*bl* 80	3 (9)
1	*fl* 80	3 (9)	*bl* 70	3 (9)

The number of experiments on each point is 6.

In Case I the probable error for direction varies from ± 2% to 6% and that for distance from 0 to ± 1%.

CASE II.

Distance of the secondary coil for the right telephone.	Usual localization. Direction.	Distance.		Occasional localization. Direction.	Distance.	
12cm	*bl* 70°	7.3sun	(22cm)			
11	*bl* 70	8	(24)			
10	*fl* 45	10	(30)	*bl* 70°	8sun	(24cm)
9	*bl* 35	7.3	(22)	*fl* 20	10	(30)
8	*f* or *b*	10	(30)	*fr* or *br* 20	8	(24)
7	*fr* or *br* 30	8.5	(25)	*f* or *b*	10	(30)
6	*br* 46.7	6.1	(18)			
5	*br* 46.7	4.3	(13)			
4	*br* 53.3	3.6	(11)			
3	*br* 60	3.3	(10)			
2	*br* 63.3	2.8	(8)			
1	*br* 66.7	3.2	(10)			

In Case II the probable error for direction varies from 0 to ± 12% and that for distance from 0 to ± 6%.

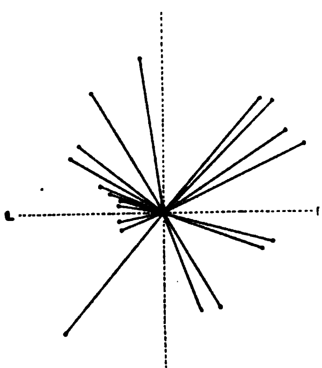

FIG. 13.

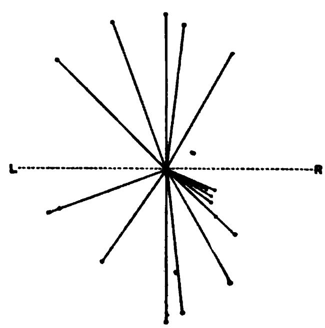

FIG. 14.

Figures 13 and 14 show these results diagrammatically.

The results of the experiments under condition 4 are given in Table XIV.

Figures 15 and 16 show the results graphically.

These results show that the field of localization of the perceived sound covered larger sectors in the experiments under condition 3. than in the experiments under condition 4. In the former it covered a sector included between *fr* 64° and *fl* 80° in front and a sector included between *br* 75° and *bl* 80° in back, while in the latter it covered a sector included between *fr* 55° and *fl* 56.7° in front and a sector included between *br* 60° and *bl* 60° in back.

TABLE XIV.

CASE I.

Distance of the secondary coil for the left telephone.	Usual localization.		Occasional localization.	
	Direction.	Distance.	Direction.	Distance.
12cm	*br* 50°	9mm (27cm)		
11	*fr* 35	9 (27)	*br* 50°	8mm (24cm)
10	*br* 43.3	8 (24)		
9	*fr* 35	9 (27)	*fr* or *br* 35	9 (27)
8	*fr* 30	9 (27)	*br* 30	8 (24)
7	*f* or *b*	8.7 (26)		
6	*fl* 20	9 (27)	*bl* 30	7 (21)
5	*fl* 43.3	6.8 (20)		
4	*fl* 45	5.5 (16)	*bl* 30	5 (15)
3	*fl* 45	5.5 (16)	*fl* or *bl* 50	5 (15)
2	*fl* 43.3	4.2 (13)		
1	*fl* 45	4.5 (13)	*fl* or *bl* 50	3.5 (10)

The number of experiments on each point is 3.

In Case I the probable error for direction varies from 0 to ± 65% and that for distance from 0 to ± 28%.

CASE II.

Distance of the secondary coil for the right telephone.	Usual localization.		Occasional localization.	
	Direction.	Distance.	Direction.	Distance.
12cm	*fl* 40°	8mm (24cm)	*bl* 60°	7mm (21cm)
11	*fl* 53.3	7.7 (23)		
10	*fl* 56.7	7 (21)		
9	*fl* 50	7.3 (22)		
8	*b* or *k*	7 (21)	*fl* 40 ; *bl* 30	{ 8 (24) / 6 (18) }
7	*f*	11 (33)		
6	*fr* 55	6.5 (19)	*br* 40	5 (15)
5	*fr* 45	6 (18)	*br* 40	6 (18)
4	*fr* 45	6.5 (19)	*br* 50	4 (12)
3	*br* 40	4.5 (13)	*fr* 30	4 (12)
2	*fr* 45	4 (12)	*br* 40	3 (9)
1	*fr* 40	3.3 (10)	*br* 50	3 (9)

In Case II the probable error for direction varies from 0 to ± 8% and that for distance from 0 to ± 15%.

Our expectation that the acoustic field would be contracted more as the two sources of sounds approached more to the median plane, and consequently the relative difference between the intensities of the sensations in the two ears would grow less, was realized by the second group of experiments. As to the forward and backward projection of the perceived sounds the above results resemble those of the first group of experiments,

where it was asserted that the cause of such forward and backward localization must be sought in the equality of the relation between the intensities with which the sounds localized in the two directions in question are received by the two ears respectively. In a similar way we will say here that the forward and backward projection in the experiments under conditions 3. and 4. originates in the resemblance which exists between the relative differences in the intensities with which the two sounds at the

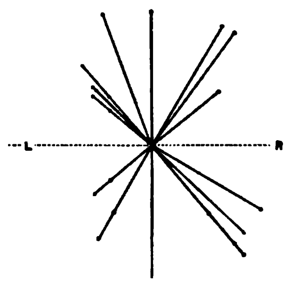

FIG. 15.

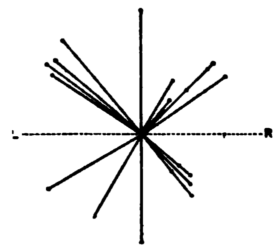

FIG. 16.

back are heard by the two ears and the relative difference in the intensities with which the two sounds at the corresponding points in front are heard by the two ears, for otherwise the sounds would never be localized both in front and in back in such a confused way as shown in the above tables.

3. Previous Investigations.

In going over a large number of monographs that treat of the localization of a sound as depending on the intensities with which the vibratory-movements affect the two ears we find several that are of special importance.

We must first notice SEEBECK's[1] computation of the intensities with which the air-vibrations from a sounding object reach the ears.

Let us suppose that the right ear is turned to a sonorous body and the left toward a wall. Supposing the wave-length of the sound to be λ, the equation of the vibratory movement will be

$$y = a \cos 2\pi \left(\frac{c\,t}{\lambda} + \tau\right),$$

where c represents the velocity of the propagation of the sound, t the time, y the extent to which an air particle is displaced at the moment t,

[1] SEEBECK, *Die Zurückwerfung des Schalles*, Annalen d. Physik u. Chemie (Poggendorff), 1846 LVXIII 465.

a the maximum value of y, and τ a constant time-value indicating the phase. If the time be counted from the moment at which the value for the direct wave is greatest in the right ear, then the vibration will be expressed by

$$y = a \cos 2\pi \frac{ct}{\lambda}.$$

The direct wave reaches the left ear in two ways. The first way is through the air whereby the sound-waves which have proceeded to the head are bent round to the ear. Let d represent the additional distance travelled by the sound-wave in reaching the left ear around the head after having arrived at the right ear. Then the vibration in the left ear will be expressed by

$$y' = -k\, a \cos 2\pi \frac{ct-d}{\lambda},$$

where the minus sign expresses the change in the phase and k is a positive number, which expresses the weakening of the sound due to the refraction around the head.

In addition to the refracted portion the ear receives vibrations transmitted from the right side to the left ear through the sôlid parts of the head. If $\frac{e}{\lambda}$ be the relation between this route and the wave-length—the latter being referred not to the air but to the solid bony substance, then $\frac{e}{\lambda}$ is a very small quantity for all usual tones. The second part of the wave in the left ear will be expressed by

$$y'' = i \cos 2\pi \frac{ct-e}{\lambda}$$

where i indicates the weakening which the sound suffers on this second route. If we add together these two components for the left ear, we have

$$y''' = -ka \cos 2\pi \frac{ct-d}{\lambda} + ia \cos 2\pi \frac{ct-e}{\lambda} = pa \cos 2\pi \frac{ct-\delta}{\lambda},$$

where $$p = \sqrt{k^2 + i^2 - 2\, ik \cos 2\pi \frac{d-e}{\lambda}}$$

and $$\cos 2\pi \frac{\delta}{\lambda} = \frac{k \cos 2\pi \frac{d}{\lambda} - i \cos 2\pi \frac{l}{\lambda}}{p}$$

As for the reflected wave, it will be expressed for the left ear by

$$y^{\text{iv}} = -qa \cos 2\pi \frac{ct-l}{\lambda},$$

and for the right ear by

$$y^r = + qa \cos 2\pi \frac{ct - l - \delta}{\lambda},$$

where the factor $-q$ indicates the weakening of the sound and the change in the phase produced by the reflection, and l the distance from the median plane of the head to the wall.

If we now add the elements which proceed from the direct and reflected waves, we obtain for the right ear

$$y^r = a_1 \cos 2\pi \left(\frac{ct}{\lambda} + \tau_1\right)$$

and for the left

$$y^l = a_{11} \cos 2\pi \left(\frac{ct}{\lambda} + \tau_{11}\right),$$

where the phase differences τ_1 and τ_{11} do not concern us for the present purpose, but a_1 and a_{11} are determined by the equations

$$a_1^2 = a^2 \left(1 + p^2q^2 + 2pq \cos 2\pi \frac{2l + \delta}{\lambda}\right)$$

$$a_{11}^2 = a^2 \left(p^2 + q^2 + 2pq \cos 2\pi \frac{2l - \delta}{\lambda}\right).$$

These quantities give the intensities of the two physical sounds in the two ears respectively, for the latter are to be measured by the squares of the amplitudes.

It should be in criticism that the direct transmission of the sound-waves through the skull is an utterly negligible quantity, but that the transmission from one tympanum to the other through the skull is a very important, though undeterminable, factor.

We must next consider STEINHAUSER'S theory of binaural audition.[1] He worked out on geometrical principles the laws which determine the relative intensities with which a sound will reach the two ears. According to him the pinna acts as a funnel to conduct into the ear those waves of sound which in consequence of their direction reach but could not otherwise enter it.

The regions of direct, indirect, and mixed hearing were distinguished according to the nature of the path by which the sound waves reach the

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[1] STEINHAUSER, *The theory of binaural audition*, Phil. Mag., 1879 (5) VII 181, 261. (This is a translation of STEINHAUSER, *Die Theorie d. binauralen Hörens*, Wien 1877.)

ears. In direct hearing the waves proceeding from the sonorous body reach the ear in a straight line and enter the auditory meatus directly. In indirect hearing the waves proceeding from the source of sound do not reach the ear in straight lines, but after undergoing reflection from external objects. In mixed hearing the waves reach the ear partly directly and partly indirectly.

The formula deduced by STEINHAUSER for direct hearing is as follows: Let β be the angle between the plane of the pinna and the line of sight, and α the angle which the line of sight makes with the direction of the sound; and let i_1 and i_2 represent the relative intensities with which the sound is heard in the two ears; then

$$\tan \alpha : \tan \beta :: i_1 - i_2 : i_1 + i_2 .$$

The direction in which a source of sound is situated may, therefore, be, according to STEINHAUSER, estimated by the different intensities with which a sound is perceived in the two ears. From the above formula the case can be deduced, in which $i_1 = i_2$. We have then $\tan \alpha = 0$ and hence $\alpha = 0$. This is the case in which the sound is just in the direction of the line of sight.

If a source of sound is situated in the region of indirect hearing, no waves of sound can reach the surface of either of the pinnæ directly; the sound produced by the sonorous body can evoke a sensation as the results of reflection, provided we neglect the possible conduction of sound through solid bodies and by refraction around the head and pinnæ. Let α be the angle which the rays of sound make with the line of sight before reflection, α_r the angle they make after reflection, and φ be the complementary of the angle which the line of sight makes with the surface which reflects the rays of sound; then by a geometrical operation

$$\alpha_r = 2\varphi - \alpha.$$

That which is heard, therefore, indirectly in the direction α makes the same impression as that heard directly in the direction α_r;. in which case α_r, whose value is dependent on φ, may, without any change of the direction of the waves of sound, assume an indefinite number of different values, since the position of the reflecting surface may as well be any other than it is, or there may be many reflecting surfaces.

If a source of sound is situated in the region of mixed hearing, then the direct rays of sound can reach only one of the two pinnæ, while both may be reached by the indirect rays. Accordingly let i_1 be the intensity with which the direct rays of sound affect one ear, and ρ_1 the increment of that intensity due to the effect of reflexion. Let ρ_2 be the

intensity of the sensation in the right ear, due to the reflexion alone. Then, on summing up the indirect and direct effects, the following equation is obtained:

$$\tan\alpha = \frac{i_1 + \rho_1 - \rho_2}{i_1 + \rho_1 + \rho_2}\tan\beta.$$

Hence, by calculation, we can find the angle within the region of direct hearing in which the source of sound is erroneously imagined to lie.

STEINHAUSER devised an instrument called homophone, wherewith to test his theory. It consisted of a system of wooden tubes for bringing to the ears the sounds of two organ-pipes tuned to unison, whose respective intensities could be regulated by stop cocks. It was held by its inventor to confirm his theory.

The observation that by binaural audition the image of the perceived sound is localized apparently in the occipital region of the head was made first by PURKYNĚ.[1] In his experiments it was shown that when a tone was conducted simultaneously to the two ears separately by means of rubber tubes the acoustic image was not perceived in the ears, but was perceived in the occipital region of the interior of the head.

An apparently similar localization of the sound in the occipital or frontal region was observed by several subsequent investigators, such as S. P. THOMPSON,[2] PLUMAUDON,[3] URBANTSCHITSCH,[4] and KESSEL.[5]

POLITZER[6] emphasized the fact that for the perception of the direction of a sound the fact of binaural hearing was requisite. He made numerous experiments upon normal and abnormal persons and found that in monaural audition the sound was localized on the side of the open ear. When a watch was moved in the horizontal plane and heard with one ear closed, the tick-tick was localized on the side of the open ear even when the watch was moved some distance farther to the other side of the median plane. The perception of the position of the sound became more difficult when the sound was moved further toward the closed ear. In the case of persons who suffered from diseases of the ears a similar error was observed, a mistake of 180° for perceiving the direction of a sound being often made. The diminution of the localizing power was

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[1] PURKYNĚ, Prager Vierteljahresschrift, 1860 III 94.

[2] THOMPSON, *On binaural audition*, Phil. Mag., 1877 (5) IV 274; 1877 (5) VI 283.

[3] PLUMAUDON, The Telegraphic Journal, London, Sept. 1879.

[4] URBANTSCHITSCH, *Zur Lehre von der Schallempfindung*, Arch. f. d. ges. Physiol. (Pflüger), 1881 XXIV 574.

[5] KESSEL, *Ueber die Function der Ohrmuschel bei den Raumwahrnehmungen*, Arch. f. Ohrenheilk., 1882 XVIII 120.

[6] POLITZER, *Studien über die Paracusis loci*, Arch. f. Ohrenheil., 1876 XI 231.

observed also in persons who suffered from hard hearing of one or both ears. With many persons the mistakes of localization were first noticed when they were subjected to a special test. He ascertained also that in binaural audition the localization was especially defective for a sound in the median plane, where the source of sound was equally distant from the two ears.

THOMPSON noticed, during his experiments on binaural hearing, an acoustic illusion due to the fatigue of the ear. One ear was fatigued by listening to a loud pure tone, and then the listener tried to estimate the direction of a sound of the same pitch. If his left ear were fatigued he would invariably imagine the source of the sound to be further to the right than it really was, and likewise the reverse. The illusory displacement in the direction of the sound was greater as the fatigue was more complete. But as the sounds of different pitch have to stimulate different fibres of the basilar membrane of the cochlea, it would be expected that the fatigue produced by a sound of a certain pitch would have no effect on the perception of a sound of another pitch. According to THOMPSON's experiments, when one ear was fatigued with a c'' fork no illusory displacement was perceived in an a'' fork.

TARCHANOFF[1] found that when telephones were held opposite the ears and intermittent currents were sent through them, the perceived sound was localized in the median plane and that it was perceived outside the plane when there was the slightest difference between the intensities of of the two sounds.

URBANTSCHITSCH[2] found that when one and the same sound of a certain intensity was led into the two ears separately by means of a T tube, one group of his observers perceived the sounds as being in the right and left ears with equal intensities, whereas another group perceived the sounds in the right and the left temporal regions of the head. When the intensity increased, these two sounds seemed to expand and approached nearer to the middle of the head, being finally brought into fusion at a sufficient intensity. Sometimes besides the two sounds a third sound was observed in the center of the head; the latter was perceived after repeated experiments or only when the observers paid special attention to it. A large proportion of the observers, when very attentive, perceived the sound—not in the ears but in the interior of the head. Finally, there were some observers by whom the sound was not generally located in the head, but projected in front, to the back, or above.

[1] TARCHANOFF, St. Petersburger med. Wochenschrift, 1878, No. 43.

[2] URBANTSCHITSCH, *Zur Lehre von der Schallempfindung*, Archiv f. d. ges. Physiol. (Pflüger), 1881 XXIV 574.

In regard to the intercranial localization there were some individual differences as to the distinct position of the perceived sound. Some localized it in the occipital region of the head, others in the frontal region or in the forehead, while still others localized it in the nose or in the pharynx. URBANTSCHITSCH asserted also that the degree of fatigue of the ear could possibly be determined by observing the position of an acoustic image. At first a very strong sound was conducted to one ear for a while; then weaker sound of the same pitch was conducted to the two ears simultaneously. The acoustic image for the latter case was found at first in the ear which was not fatigued and remained there for a few seconds; then the image travelled gradually towards the median plane and at last was found at the centre of the head. URBANTSCHITSCH explained the phenomenon in the following way. As one ear was very strongly stimulated in the beginning it was fatigued for some interval of time and the other ear, which was not stimulated, became relatively sharper. Consequently, when a weaker sound was conducted to the two ears simultaneously the sensation in the latter ear was evidently stronger and the perceived sound was located on the side of that ear. But the other ear began to recover gradually, and at the same time the sharper ear began to be fatigued. As a result, the perceived sound began to travel towards the median plane and at last reached the center of the head when the sharpness of the two ears became the same.

KESSEL[1] performed his experiments by inserting tubes from a funnel shaped sound-receiver into the ears, thus excluding the action of the pinnæ. The sound of a tuning fork was conducted into the two ears by this apparatus. When the tubes were of the same length and were opened equally wide the ears were stimulated with the same strength and the resulting single sound was located in the median plane of the head. If one of the tubes was more or less pressed so that the two ears were stimulated differently, then the sound was perceived in the ear which was stimulated more strongly.

KESSEL made another interesting experiment. According to him the principle of the localization of a sound on the side of a stronger excitation holds good even in the case when one ear is not directly stimulated by the waves of the sound, but stimulated by reflected waves. To prove this he hung a watch in the axis of a parabolic mirror; the head of the observer was adjusted between the mirror and the watch so that the two ears were opposite them. Then the watch was so adjusted that the reflected rays, which were gathered at the focus, would stimulate the ear oppo-

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[1] KESSEL, *Ueber die Function der Ohrmuschel bei den Raumwahrnehmungen*, Arch. f. Ohrenheilk., 1882 XVIII 120.

site the focus more strongly, in which case the sound was localized in the direction of the mirror. If the watch was brought nearer to the ear on its side, so that the direct rays of the sound would stimulate this ear more strongly, then the sound would be localized on the side of the watch.

ROGDESTWENSKY[1], one of TARCHANOFF'S pupils, produced sounds at symmetrical points, and observed that the perceived sounds were localized in the head, breast and abdomen according to the difference in the height of the symmetrical points.

To exclude the function of one ear in perceiving the direction of a sound, PREYER[2] made experiments by using a telephone instead of the snapper sounder. The intensity of the telephone sound was weakened by reducing the intensity of the electric current so that by stopping the ears the sound could not be heard by the subject. Then one ear was opened so that the sound was heard by that ear only. The result of the experiments showed that under such conditions errors occurred which were not observed in ordinary perception, and it was often very difficult, even with a strenuous attention, to get rid of these errors. These errors were localisations on the wrong side.

Similar experiments were afterwards made by ARNHEIM[3]. In his experiments the number of correct perceptions with one ear amounted to only 22% of the total, while with both ears it amounted to 39.7% (in PREYER'S experiments 30%). He noticed also a decided tendency to locate the perceived sound on the side of the open ear.

SCHAEFER,[4] who had made elaborate experiments on the perception of the direction of sound in coöperation with PREYER, attacked a peculiar side of the problem at a later date, namely, the localization of beats and difference tones. His results may be summarized as follows.

If the relative intensity of the primary tones is equal, then the beats appear to proceed from the region between the points at which the two tones are situated whether they are on the same or different sides of the median plane. The localization of the beats in the median plane when the primary tones of equal intensity are situated on both sides of the plane, is a special case of this general fact. SCHAEFER thinks it clear that when the two forks are placed on the same side of the median plane the

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[1] ROGDESTWENSKY, *Ueber die Localisation der Gehörsempfindungen*, Diss. 1887.

[2] PREYER, *Die Wahrnehmung der Schallrichtung mittelst der Bogengänge*, Archiv f. d. ges. Physiol. (Pflüger), 1887 XL 586.

[3] ARNHEIM, *Beiträge zur Theorie von Schallempfindungen mittelest der Bogengänge*, Diss., Jena 1887.

[4] SCHAEFER, *Ueber die Wahrnehmung und Localisation von Schwebungen und Differenztönen*, Zt. f. Psych. u. Physiol., 1890 I 81.

beats are perceived more strongly by the ear on that side; and that the beats are perceived by the two ears with equal intensity when the sources of the tones are found in the median plane. For sounds located on different sides of the median plane it can be, according to SCHAEFER, mathematically proven that the intensity of the beats is equal on the two sides when the relative intensities of the primary tones are equal, but on the contrary the intensity of the beats is stronger on the side of the stronger primary tone when the relative intensities of the primary tones are not equal. SCHAEFER's conclusion is that the beats will be localized on the side of the ear which is more strongly excited by them, but in the median plane if the two ears are equally excited by them. The further exact determination of the direction is dependent upon that of the relatively stronger primary tone. The localization of beats is, therefore, governed by the same principles as the localization of simpler sounds, i. e., the localization on the side of the ear which is excited more strongly, and the localization of sounds in the median plane when the two ears are excited with equal intensity.

As for the perception of difference tones, the localization is apparently contradicted by that of beats. For when two forks of unequal intensities are placed on the different sides of the median plane the difference tones are heard on the side of the weaker primary tone. This is not, however, really contradicted by the localization of the beats on the side of the stronger primary tone. For the localization of difference tones on the side of the weaker primary tone is based upon the fact that on this side the relation of intensity which is more favorable for the perception of the difference tones predominates, and the difference tones are heard louder on this side, for if the left sound, for example, is relatively stronger, the left ear is made "physiologically deaf" for the sound coming from the right side, and thereby the perception of the difference tone is made impossible. Difference tones are, therefore, localized after all on the side of the ear which is excited more strongly. As for the median localization of the difference tones, the result is similar to that of the beats, for when the two primary tones of equal intensity reach the ears from the two sides of the median plane either by air transmission or by cranial conduction, the difference tones are localized in the median plane.

Finally, one more phenomenon which was emphasized by SCHAEFER[1] is to be mentioned. If a fork be placed on the top of the head the sound will be localized in the median plane, but it will shift to one ear if that ear be closed. SCHAEFER explained this phenomenon

[1] SCHAEFER, *Ein Versuch über die intrakranielle Leitungleises ter Töne von Ohr zu Ohr*, Zt. f. Psych. u. Physiol. d. Sinn, 1891 II 111.

in the following way. The osseous parts of the auditory apparatus, the tympanum, and finally the air in the external auditory tube will be put into vibration by the waves which the sound produces in the labyrinth by means of cranial conduction. In this case, therefore, sound proceeds in just the opposite way to the usual one, where it passes from the air into the ear. Now, if we close the ear with the finger, the tympanum will be put into stronger vibration on account of the reflexion of the waves from the finger. Consequently the component sound is stronger in the closed ear and the perceived sound will appear to shift from the median plane toward the side of the closed ear. The same effect will be produced if we apply a resonator to the ear either by holding by fingers or by supporting on a stand. To this same class belongs another phenomenon which is noticed by many persons of normal hearing. When we sing loudly a low tone like the German "u" and stop one ear, but not very tightly, the tone will move from the initial position in the larynx to the stopped ear, but it will move again to the median plane in the interior of the head if the other ear is stopped in the same way.

Bloch[1] investigated both binaural and monaural localization by measuring the least noticeable change in the position of sounding body. His conclusion runs as follows.

The most important function of binaural audition is the perception of the direction of a sound; the perception of direction is more accurate in the horizontal and in the frontal planes than in the median plane; in the former two planes the localization is based chiefly upon the relative difference between the intensities of the sounds heard by the two ears, and, secondarily, upon the change in the intensity of the perceived sound which arises from the influence of the pinnæ; and in the median plane the action of the pinnæ which collect the sound-waves into the auditory meatus is the chief condition for the localization of sounds.

These investigations make it clear that the relation of intensity between the two components of a sound heard by the two ears is a fundamental—or the fundamental—factor of localization in regard to direction. My own work, reported in this section, aimed to further define this factor and its effect.

III. Localization of the perceived sound at the middle point between the sources of two objective sounds.

In the foregoing experiments the two telephones were restricted to the same primary plane at two points.

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[1] Bloch, *Das binaurale Hören*, Zt. f. Ohrenheilk., 1893 XXIV 25.

In the following experiments the telephones were placed at various points on the surface of the spherical cage (Fig. 1).

(*a*) One of two telephones was situated in the median plane and the other in the frontal plane. The two telephones were placed at a same level. The physical intensities of the two sounds were kept as far as possible equal and constant during the experiments. The positions of the two telephones were as follows:

TABLE XV.

Number of position.	Position of the two telephones.
1	*fo* and *ro*
2	*fo* " *lo*
3	*fu* " *ru*
4	*fu* " *lu*
5	*bo* " *ro*
6	*bo* " *lo*
7	*bu* " *ru*
8	*bu* " *lu*
9	*f* " *r*
10	*f* " *l*
11	*b* " *r*
12	*b* " *l*

On the one hand, if a sound comes from the median plane only, the two ears will be stimulated equally, and consequently the perceived sound will be located in the median plane. On the other hand, if a sound comes from the frontal plane only, the two ears will be stimulated with the maximum relative difference in intensity, so far as the sounds at a same level are concerned, and consequently the perceived sound will be located nearly in the frontal plane. Now, if the two ears are stimulated simultaneously, as in our experiments by the two sounds situated in the above two planes, the relation between the intensities of the sounds heard by the two ears will be just like the relation between the intensities with which the two ears hear a sound coming from the plane which lies just between the above two planes. The perceived sound may accordingly be expected to be located in the plane between the median and frontal planes. Moreover, in our experiments, as the two telephones lie on the same level the perceived sound may be expected to be mostly localized on that level.

These expectations were fully realized by the actual results which are given in Table XVI. Mr. K. Matsumoto, a graduate student of psychology, and Mr. T. Nakashima were the subjects of the experiments.

To make the comparison between expectation and realization easier, I have arranged the results as in Table XVII. The symbols in

TABLE XVI.

Localization.

	Observer T. N.	Observer K. M.
1	*for, for, for, fo(r), o(r), fur.*	*for, (f)or, o(r) o, o(b), bor (k).*
2	*fol, fol, fol, o, o(b), b(o)l.*	*fol, ol, ol, ol, o(b), o.*
3	*fur, fur, fur, fur, (f)ur, bor (k).*	*fur, fur, bor, bor, b(u)r.*
4	*ful, ful, ful, ful, ful, ul, b(u)l.*	*ful, ful, bol, bo(l), bol.*
5	*bor, bor, for, for, fo(r), fur.*	*bor, bor (k), bor (k), for, fo, o(k).*
6	*bol, bol, b, o, ful, ful, (f)ul.*	*bol, bo, bo, lo, o, bo, fol.*
7	*fur, fur, fur, fur, ur.*	*bur, bur, bur, bur, (b)ur, hor, hor, ho, ur.*
8	*bul, bul, (b)ul, ul, (f)ul.*	*bul, bul, (b)ul, ul, bol, bol.*
9	*fr, fr, fr, f(k), fo(r)(k), b(r).*	*bor (k), bor (k), bor.*
10	*fl, fl, fl(u), bol, br, br, br, bor, bor.*	
11	*b(k), b* or *f, bl, l, b, bol(k), bol.*	
12	*fl.*	

TABLE XVII.

	A Expected localization.	*B* Usual localization	*C* Occasional localization.	*D* Rare localization.
1	*for*	*for*	*bor, bur*	
2	*fol*	*fol*	*bol*	
3	*fur*	*fur*	*bur, bor*	
4	*ful*	*ful*	*bul, bol*	
5	*bor*	*bor*	*for*	
6	*bol*	*bol*	*ful*	
7	*bur*	*bur*	*fur*	
8	*bul*	*bul*	*bol*	
9	*fr*	*fr*		
10	*fl*	*fl*		*for, bor*
11	*br*	*br*		*bol*
12	*bl*	*bl*	*fl*	*bor*

column *A* show the directions in which the perceived sounds are theoretically expected to be localized; the symbols in column *B* the directions in which most of the perceived sounds were actually localized, and

the symbols in columns C and D the directions in which the perceived sounds were sometimes localized. The striking correspondence between A and B proves the correctness of our view. It is also very interesting to note that the perceived sounds were sometimes located in the directions in column C instead of the directions in column B. There is a reasonable justification for such localizations. When we are stimulated by a sound from the median plane the intensities of the sounds heard by the two ears are equal whether the sound is situated in front or in back. Subjective discrimination between front and back is, as we will see later, based chiefly upon the difference in absolute intensity, for the sound coming from the front is heard more strongly than the same sound from the back. So if the sound from the front in the median plane be weakened during the experiment by fluctuation of the electric current, while the ears are stimulated at the same time by the sound in the frontal plane, it will be quite possible that the relation of intensities will be like the relation of intensities with which the observer hears the sounds from the frontal plane and from the back part of the median plane. In such a case the observer may locate the sound to the rear instead of locating it to the front. This appears to be the reason why in the above experiments the perceived sounds were sometimes located at *bor* or *bol* instead of *for* or *fol*; at *bur* or *bul* instead of *fur* or *ful*. For the similar reasons the perceived sounds were located at *ful* or *fur* instead of *bul* or *bur*; and at *fl* instead of *bl*. These results agree with the confusion between front and back which we have frequently observed in our previous experiments.

Besides the confusion between front and back we find here another kind of confusion; above and below are sometimes confused with each other. In the above experiments the observer located the sound at *fur* when *for* was expected; at *bor* or *bol* when *bur* or *bul* was expected. This kind of confusion can be explained by the function of the pinnæ. It is found that on account of the peculiar shape of the pinnæ a sound coming from *ro* or *lo* is perceived by the two ears with almost the same relative difference of intensity as a sound coming from *ru* or *lu* and a sound coming from *fu* is perceived with almost the same intensity as a sound coming from *fo*. So when we are stimulated simultaneously by sounds coming from *fo* and *ro* it is quite possible that the relation between the intensities of the sounds in the two ears will be like the relation between the intensities with which the sounds coming from *fu* and *ru* are heard by the two ears. In such a case the perceived sound may be located at *fur* instead of *for*. Moreover, the fluctuation in the electrical current will have some effect in producing the confusion. Such being the

fact, we have reasonable justification, under the above conditions, for occasionally localizing the perceived sounds in the directions in column *C*.

As for the localizations of the sounds in the directions in column *D*, they occurred very rarely and could be ascribed both to the inaccuracy of the experiment and inattention of the observer.

(*b*) The localization of the perceived sound at the middle point between the sources of the two component sounds is not restricted to the case in which the components sounds are put in the primary planes and at the same level. In the following experiments I placed two component sounds at different levels and one of them in a secondary plane, namely, one telephone was placed at the terminus of a secondary axis in the horizontal plane and the other telephone was placed at the terminus of a secondary axis in the frontal or median plane. The objective positions of the telephones were, therefore, as indicated in Table XVIII.

TABLE XVIII.

Number of position.	Positions of the two telephones.	Number of position.	Positions of the two telephones.
1	*fr* and *ru*	9	*fr* and *ro*
2	*r* " *fu*	10	*fr* " *fo*
3	*fl* " *lu*	11	*fl* " *lo*
4	*fl* " *fu*	12	*fl* " *fo*
5	*br* " *ru*	13	*br* " *ro*
6	*br* " *bu*	14	*br* " *bo*
7	*bl* " *lu*	15	*bl* " *lo*
8	*bl* " *bu*	16	*bl* " *bo*

The experiments were made upon T. N. and the results were as given in Table XIX.

TABLE XIX.

	Localization.		Localization.
1	*fr(u), fr, fr, fr, fr, fr, fr.*	9	*f(o)r, f(o)r, f(o)r, fr, fr, fr, fr.*
2	*fr(u), bur(k), fr, fr, fr, fr, fr.*	10	*fo(r), fo(r), f(or), f(r), fr.*
3	*fl(u), fl(u), fl(u), fl(u), fl(u), fl.*	11	*f(u)l, f(u)l, l(u), fl, fl, fl.*
4	*fl(u), fl(u), fl(u), fl, fl, fl.*	12	*f(u)l, fl(k), (f)l, o(k), fl, fl.*
5	*br(u), ru(b), ru(b), br, br, r(b).*	13	*(b)ur, (b)ur, (b)ur, f(u)r, bur, br, br.*
6	*bur, bur, (b)ur, b(u)r, br(k), br.*	14	*b(u)r, bu(k), b(uk), b(k), b, b* or *f.*
7	*bul, bul, bul, b(u)l, bl.*	15	*(b)ul, (f)ul, lu, lu, l.*
8	*bul, bul, bul, bul, bul(k), b(l).*	16	*b(u)k, bu, b(k), f, f(k), b(k), b(k).*

As the two telephones were situated at different levels, the localization of the perceived sound at the middle point between the sources of the two objective sounds was not so clear as in the preceding experiments. Still the results were in conformity with the preceding results, for the perceived sound was localized or tended to be localized at the middle point.

I have already mentioned that a sound coming from *ro, lo, fo* or *bo* is perceived by the two ears with almost the same intensity as a sound coming from *ru, lu; fu* or *bu* respectively. This is the reason why the perceived sound was sometimes located under the above conditions at a point on the same side at 90° away from the middle point between the sources of the two component sounds, i. e., at the corresponding point in back instead of front, above instead of below and vice versa.

(*c*) In the preceding two groups of experiments the two objective sources of sounds were kept unmoved during the experiment. If the perceived sound is located under such a statical condition at the middle point between the two objective sounds it may also be expected that if the two sounds are moved continuously during the experiment the perceived sound will move, too, in the direction resultant to the two directions along which the two sounds are moved.

TABLE XX.

	Directions in which the two telephones were moved.		Directions in which the two telephones were moved.
1	*fl* { *ful* / *f* }	13	*fr* { *for* / *f* }
2	*fl* { *ful* / *l* }	14	*fr* { *for* / *r* }
3	*bl* { *bul* / *b* }	15	*br* { *bor* / *b* }
4	*bl* { *bul* / *l* }	16	*br* { *bor* / *r* }
5	*fr* { *fur* / *f* }	17	*f* { *fr* / *fo* }
6	*fr* { *fur* / *r* }	18	*f* { *fl* / *fo* }
7	*br* { *bur* / *b* }	19	*b* { *br* / *bo* }
8	*br* { *bur* / *r* }	20	*b* { *bl* / *bo* }
9	*f* { *fr* / *fu* }	21	*fl* { *fol* / *f* }
10	*f* { *fl* / *fu* }	22	*fl* { *fl* / *l* }
11	*b* { *br* / *bu* }	23	*bl* { *bol* / *b* }
12	*b* { *bl* / *bu* }	24	*bl* { *bol* / *l* }

This was the subject of the next experiments. As is show in Table XX the two telephones were started from one and the same point of the horizontal circle, and then one telephone was moved along the horizontal circle while the other was moved along one of the two vertical circles downward or upward. For example, the telephones were started at *fl* and one was moved toward *ful* while the other was moved toward *f*. The experiments were made upon T. N.

Table XXI gives the results of the experiments. The expressions indicate the directions in which the sound appeared to move; for example, *fl–fu* indicates that the sound appeared to move from *fl* to *fu*.

TABLE XXI.

	Direction in which the perceived sound moved.		Direction in which the perceived sound moved.
1	*fl–fu, fl–fu, fl–fo, fl–f,* *fl–ful, fl–ful.*	13	*fr–fo, fr–fu,* *fr–f, fr–f, fr–f, fr–for.*
2	*fl–lu, fl–lu, fl–l(u), fl–lo,* *fl–l.*	14	*fr–ro, fr–ro, fr–r(u),* *fr–r, fr–r, fr–r(k).*
3	*bi–bu, bl–bu, bl–bu, bl–bu,* *bl–b.*	15	*br–bu,* *br–b, br–b(k), br–b(k), br–b(k).*
4	*bl–lu, bl–lu, bl–l,* *bl–l, bl–l.*	16	*br–ru, br–u* or *br–r,* *br–r, br–r, br–r.*
5	*fr–fu, fr–fu, fı–fu,* *fr–fur, fr–fur, fr–fur.*	17	*f–fur, f–fur, f–fur,* *f–fr, f–fr, f–fr(k).*
6	*fr–ru, fr–ru, fr–ru,* *fr–ru, fr–ru.*	18	*f–fol, f–ful,* *f–fl, f–fl, o–l(k), o–fo(k).*
7	*br–bu, br–bu, br–bu,* *br–bu, br–bu.*	19	*b–bur, b–bur, bo–bur, bo–bur,* *ho–b.*
8	*br–ru, hr–ru, br–ru,* *br–ru, br–r.*	20	*b–bul, b–bul,* *b–b(u)l, b–b(u)l.*
9	*f–fur, f–fur, f–fur,* *f–jur, f–jr.*	21	*fl–fu, fl–lo, fl–lo, fl–lo,* *fl–f.*
10	*f–ful, f–ful, f–ful,* *f–f(u)l, f–fl.*	22	*fl–lo, fl–lo(k), fl–fu,* *fl–l, fl–l.*
11	*b(u)–bur, b–bur, b–bur,* *bu–br, b–br.*	23	*bl–ho, bl–bu, bl–bu,* *bl–b.*
12	*b–bul, b–bul,* *bu–bl, b–bl.*	24	*bl–lu, bl–lu, bl–lu,* *bl–l, hl–l, bl–l*

When one of the two telephones was moved along the horizontal circle and the other was moved downward (i. e., cases 1 to 12) the sound appeared to move in a direction resultant to those directions along which the two telephones were moved. The results can be explained by the relative and absolute differences in the intensities of the sounds in two the ears. If one telephone moving from *fl, fr, bl* or *br to f* or *b* as in 1, 3, 5, 7 were to act alone the relative difference in the intensities of the sounds in the

two ears would decrease gradually, and consequently the sound would be perceived to move from the secondary vertical plane towards the sagittal plane. On the other hand, if the other telephone moving downward were to act alone, not only the absolute intensity of the sound, but also the relative difference between the intensities of the sounds in the two ears would grow less and less, and consequently the sound would be perceived to move downward along the secondary vertical circle. Then, if these two sounds were to act at the same time, the relation of the intensities of the sounds in the two ears would be like the relation of intensities with which a sound moving in the resultant direction would be heard by the two ears. Therefore the sound appeared in 1, 3, 5, 7 to move in the resultant direction. In 2, 4, 6, 8, 9, 10, 11 and 12 the relative difference increased gradually, for one sound was moved along the horizontal circle more towards the auditory axis, while the other sound was moved downward. Accordingly the sound appeared to travel more towards the side and at the same time more downward, i. e., along the direction resultant to those directions along which the two sounds were moved.

In the cases 13 to 26 one telephone was moved along the horizontal circle and the other was moved upward. In these cases it would be expected that the perceived sound would travel as before along the resultant direction. The results were quite perplexing, for though the sounds were sometimes perceived to travel along the resultant direction most of them were perceived, as shown in the Figure 17, to move downward along the direction nearly vertical to the resultant direction. In the figure *OI* and *OII* are the directions along which the two telephones were moved; *OA* is the direction nearly resultant to the above two directions; *OB* is the direction along which the perceived sound sometimes appeared to move.

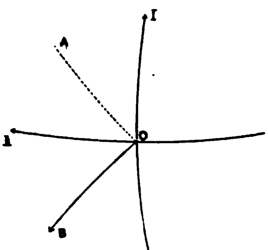

FIG. 17.

The explanation is not hard to find when we consider the fact that both the absolute intensity and the difference in the relative intensities become less and less when a sound is moved along the vertical circle upward. This decrease in intensity can be interpreted as the effect of the motion of the sound either upward or downward. If the former interpretation be taken the sound will be judged to move along the resultant direction, while if the latter be taken the sound will be judged to move along the direction nearly vertical to the resultant. Accordingly, the ob-

server may perceive the sound to move sometimes in one direction and sometimes in the other. But we must note here that the confusion is not restricted to the case in which the sound moving upward is taken for a sound moving downward, for sometimes the opposite happened as in 1 and 2, though not frequently. We must conclude that the upward and downward directions, under the condition of our experiments, are liable to be confounded with each other, owing to the similarity of the relation in the stimulation of the two ears.

The results of the experiments in this section tend to show that: (1) two component sounds of equal intensity at the same or different levels will give in combination a localization at the middle point between the two points at which the components are placed; (2) two component sounds of equal intensity which start at one and the same point and move simultaneously in different directions will give in combination a localization in the direction nearly resultant to the two directions along which the components are moved; (3) an occasional localization, under the above conditions, at the corresponding point in back instead of front, above instead of below and vice versa, arises from the confusion between front and back, above and below. All these localizations can be explained by the principle of relative and absolute intensities.

IV. Confusion between front and back.

When a source of sound is situated in the median plane the intensity of the sound heard by one ear is equal to that of the sound heard by the other ear. if the sensitiveness is the same for both ears; this is true whether the objective sound is situated to the front or to the rear. This is the cause of the confusion between front and back.

Ambiguity of the judgment as to whether a sound which is not in the median plane is to be localized in front or in back can be explained in a similar way. To one side of the observer and probably nearly in the auditory line there must be, as was noticed by Rayleigh,[1] one direction in which the ratio of the intensity of a sound as heard by one ear to the intensity of a sound as heard by the other ear has a maximum value which is greater than unity. For sounds situated in directions in front of this the ratio of the intensities has a less and less value, approaching unity as its limit when the sound is immediately in front. In like manner, for directions intermediate between the direction of maximum ratio and that immediately behind the observer, the ratio of intensities varies continuously between the same maximum value and unity. Accordingly, for

[1] Rayleigh, *Acoustical observations*, Phil. Mag., 1877 (5) III 456.

every direction in front there must be a corresponding direction behind for which the ratio of intensities has the same value; and these two directions are liable to be confounded with each other. The only directions as to which there is no ambiguity are the directions of maximum ratio itself, namely, right and left.

This view has been partly substantiated by the results of the foregoing experiments, but to make matter clearer I submitted it to the test of special experiments.

Two telephones were placed at *fr* 45° and *br* 45° on the same side of the observer at a distance of 60^{cm}. A short sound was to be given by one of them and the observer (Mr. K. Miura), a student of law, was to localize the sound. The intensity of the sound could be changed, as before, by means of the sliding inductorium. When the sound was of moderate intensity the observer could generally distinguish whether it came from the front or the back. But when the sound from the front became very weak he was liable to perceive it in the back, and projected it backward more towards the median plane when it grew weaker. Even when the sound from the front was of considerable strength he was sometimes liable to project it in the back. As for the sounds coming from the back, they were mostly projected in that direction.

A similar kind of experiment was made by using a watch instead of a telephone. The observer was seated, with his eyes closed, in the middle of a large room on a still evening. A watch was held at *fr* 45° or *br* 45° at a certain distance, and the observer was to tell the direction of the sound. In this case the result was the reverse of that for the telephone experiment. For here we found that the sound coming from the front was never localized in the back, while the sound coming from the back was frequently liable to be localized in the front, especially when it was more distant. In respect to the latter point there were some individual differences. One person projected almost all the ticks of the watch in front. Another person projected the ticks in the back when the watch was held at the distance of 50^{cm}, while he projected them in front quadrant when the watch was held at the distance of 100^{cm}. The general results of these experiments were, therefore, that when the sound coming from the back was situated near the ear, and was consequently stronger, the relative difference between the intensities of sensations in the two ears being also greater, it was generally localized in the back; whereas it tended to be localized in front when the sound was more distant and was consequently weaker, the relative difference between the intensities being also smaller. In the latter case the perceived sound tended to be localized more towards the median plane in front when the

objective point of sound was more distant and consequently the relative difference between the intensities of the sensations in the two ears was smaller, and it tended to be localized more towards the side when the objective point of sound was less distant and consequently the relative difference was greater.

RAYLEIGH conducted an experiment of similar kind by using two 256 v. d. forks and resonators, the observer being placed between them. At a given signal both forks were struck, but only one of them was held over its resonator. The observer was required to keep his head perfectly still, a very slight motion being sufficient in many cases to give the information that was previously wanting. In these experiments the observer facing north made mistakes between forks bearing approximately north-east and south-east, though he could distinguish without a moment's hesitation forks bearing east and west.

In connection with the above I may mention another kind of experiment which I conducted. From the fact that the perceived sounds were located in the median plane when two sounds were placed at symmetrical points on two sides, one on each side, I thought it possible that the same results would be obtained if two sounds were placed at diagonal points of the horizontal circle, for the relation of intensities of sounds received in the two ears might be equal in the latter case to the relation of the intensities of sounds received in the two ears in the former case. So I placed two telephones in the horizontal plane in six combinations such as: (1) *r* 22.5° and *l* 157.5°; (2) *r* 45° and *l* 135°; (3) *r* 67.5° and *l* 112.5°; (4) *r* 112.5° and *l* 67.5°; (5) *r* 135° and *l* 45°; (6) *r* 157.5° and *l* 22.5°. I found that under these conditions the localization of the perceived sound in the median plane was not so striking as was the case when the two sounds were placed at symmetrical points on the two sides of the median plane, one on each side, for though the observer located the perceived sounds at *b* or nearly at *b* for (2), (4) and (5) and at *f* or nearly at *f* and sometimes at *b* for (6), yet he located the sounds for (1) and (3) outside of the median plane. The results ran as follows: in case (1) the localizations were *b* or *for*, within the head (*bor*), within the head (*for*), within the head (*r*); in case (2) they were within the head (*b*), within the head (*b*), within the head (*b*), *b*, or *r*; in case (3) they were *b* or *r*, *fo*, *r*, *fr* 65°, *fr* 65°, *r*; in case (4) they were *b*, *b*, *b*, *b*, *b* (*u*); in case (5) they were *b*, *b*, within the head (*b*), *b* (*l*), within the head (*r*), *fl*, *fl*; in case (6) they were *f*, *f*, *f* or *b*, *f* (*l*), *f* (*l*), *f* (*l*).

On account of the comparative irregularity of these results I was doubtful whether the intensities of the sounds heard by the two ears under these

conditions were equal as I had at first thought. At any rate it was evident from these results that there was a possibility of finding two points in diagonal quadrants (i. e., quadrants at opposite ends of the same axis), one in each quadrant, which in combination will give a localization in the median plane. I learned afterwards that this possibility had been realized in the experiments conducted by MÜNSTERBERG and PIERCE[1] from which we can also conclude that the intensities of sounds heard in the two ears under the above conditions could not be regarded as exactly equal. From their experiments we learn that for any given point in either of the two quadrants upon one side of the median plane a point can be found in each of the two quadrants on the opposite side which in combination with the first will give a localization in the median plane at 0° or 180°. For example, a sound at *r* 45° will give 0° or 180° not only with its symmetrical *l* 45°, but also with a sound in the left back quadrant. Thus, for one of their observers B. *r* 45° gave 0° with *l* 105°, for another observer M. with *l* 115°, for W. with *l* 130°, for P. with *l* 140°, for N. *r* 45° gave 0° with *l* 45°, but 180° with *l* 130°, and for R. with *l* 125°.

MÜNSTERBERG and PIERCE regarded this as a special case of the more general principle : that for any given point in either of the two quadrants upon one side of the median plane a point can be found in each of the two quadrants on the opposite side which in combination with the first will give the same subjective localization. Thus their observer B. located *r* 10° + *l* 110° and *r* 10° + *l* 70° at *l* 20°; *r* 50° + *l* 10° and *r* 50° + *l* 130° at *r* 20°; *r* 100° + *l* 50° and *r* 100 + *l* 150° at 125°; *r* 120° + *l* 40° and *r* 120° + *l* 100° at *r* 40°. The results were similar with other combinations. Again, according to them, very similar to this principle is the fact that different individuals at different times locate a given combination in two different quadrants. Thus B. locates 0° + *l* 110° at *l* 60° and again at *l* 130°; *r* 30° + *l* 110° at *l* 40° and *l* 160°, etc. We may give the following as an illustration of the individual differences : sounds at 0° + *r* 135° by B. *r* 25°, by M. *r* 65°, by P. *r* 160°; sounds at 0° + *r* 160° by B. *r* 170°, by M. *r* 75°, by P. *r* 10°. The basis of these differences lies, they say, in the fact that not only 0° and 180°, but also other points before and behind, are confused when they are sounding in a combination. In the example 0° + *r* 135°, for instance, the judgment *r* 65° represents the middle ; *r* 25° represents the middle, if *r* 135° is confused with the corresponding sound from the front at 45°; and *r* 160° represents the middle, if 0° is confused with 180°. Just so

[1] MÜNSTERBERG and PIERCE, *The localization of sound*, Psychol. Rev., 1894 I 461.

with $0° + r\,160°$, $r\,170°$ results if $0°$ stands for $180°$; and $r\,10°$ if $r\,20°$ stands for $r\,160°$.

All the foregoing results show that for a point given in the front quadrant a corresponding point can be found in the back quadrant on the same side of the median plane, which is liable to be confused with the first; and that the ambiguity or uncertainty of the judgment as to front and back is based upon the similarity which exists between the relation of stimulation of the ears by a sound in one quadrant and the relation of stimulation of the ears by the same sound in the other quadrant.

The discrimination between front and back seems to be based upon the absolute intensity, pitch and duration of the sound, to which the tactual sensations of the pinnæ and head may give some help, though the latter can not be made clear by experiment. The dependence of the discrimination upon the former was investigated by BLOCH,[1] as far as the discrimination between *f* and *b* is concerned. We can accept his results without further discussion, though they may not be applied to the discrimination between front and back in general. They are as follows.

BLOCH gave sounds at *f* and *b* respectively and made his observer judge from which of these two directions the sounds came. The results show that the correctness of judgment depends upon the pitch, intensity, duration and distance of the sound. When he used a tuning fork of 220 v. d. it seemed clear in general that a loud and long sound at the distance of 1^m was correctly judged as well in front as in back. A weak and short sound was not always localized correctly. A sound of greater intensity and duration—i. e., a sound of stronger acoustic excitation—made the perception of the direction in the median plane easier. When BLOCH made similar experiments with a pipe having a pitch of d_3 (1188 v. d.) the sound at a greater distance was localized better. When the distance of the sound increased the sound at the back appeared considerably weakened and the discrimination between front and back became easier. In the median plane a higher tone was localized better than a lower one. Again when the click of a snapper sounder was given at a distance of 2.4^m a weaker tone tended to be located more in back and a stronger tone more in front. With the increase of the intensity of tone the number of the *f* judgments increased and the number of the *b* judgments decreased, or with the decrease of the intensity of tone the number of the *f* judgments decreased and the number of the *b* judgments increased. We learn by experience, says BLOCH, that a certain sound is perceived with less intensity when it comes from the back than when it comes from the front. Accordingly when the direction of the sound is not

[1] BLOCH, *Das binaurale Hören*, Zt. für Ohrenheilk., 1893 XXIV 25.

clear we tend to locate a stronger sound in front and a weaker one in back.

In connection with the experiments on the confusion between front and back other experiments of somewhat similar kind may be mentioned. We have already seen that the value of the relative difference between the intensities with which a sound is heard by the two ears varies according to the direction of the sound. But the direction of the sound seems not to be the only condition upon which the change in the relative difference depends, for this difference seems also to depend upon the absolute intensity of the sound. In other words, this value seems to change, other things being equal, when the intensity of the sound changes. It has been a well-known fact since FECHNER's[1] experiments that when two unison forks are held before the two ears respectively and one of them is more strongly sounded than the other, the single resulting sound appears to the subject to be heard entirely by the ear on the side of the stronger component. The ear which receives the weaker sound is said to become more or less "physiologically deaf." It seems to me that this "physiological deafness" of one ear becomes relatively greater when the sound received in the other ear grows stronger, and thereby the perceived sound tends to be projected much more towards the side on which the source of stronger sound is situated than when a sound of weaker intensity is used. The following experiments were designed to make this point clearer.

TABLE XXII.

Distance of the secondary coil for the back telephone.	Judgment of direction.	Judgment of distance.	
10^{cm}	*fr* 80°	8.5 ^{sun}	(25^{cm})
9	*fr* 80	9	(27)
8	*fr* 85	8.5	(25)
7	*r* 90	8.5	(25)
6	*r* 90	8.3	(25)
5	*br* 82.5	7	(21)
4	*br* 80	6.5	(19)
3	*br* 80	5.3	(16)
2	*br* 80	5	(15)
1	*br* 80	4.3	(13)
0.6	*br* 80	4	(12)

The number of experiments on each point is 4. The probable error for direction varies from 0 to $\pm 1\frac{9}{10}$% and that for distance from 0 to $3\frac{1}{61}$%.

[1] FECHNER, *Ueber einige Verhältnisse des binocularen Sehens*, Abhl. d. k.-sächs. Ges. d. Wiss., math.-phys. Cl., 1860 VII 339.

Two telephones were situated on the right side of the observer. One at 60° to the front and the other at 60° to the rear. The wires for the latter were connected with the secondary coil and the wires for the former with the primary coil of the sliding inductorium. In each experiment the intensity of the sound in front was kept constant, while that of the sound to the rear was varied. The subject of the experiments was C. W. Table XXII gives the results.

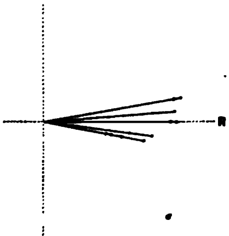

Fig. 18.

Figure 18 shows the results graphically. The perceived sound was gradually placed more towards the back as the sound to the rear grew stronger and consequently the relative difference became greater. The gradual change of the angular magnitude of the localized position of the perceived sound corresponding to the gradual change in the intensity of the sound to the rear is shown in Table XXII. An interesting point is that when the sound to the rear reached its maximum intensity—and consequently according to our view the relative difference between the intensities of the sensations in the two ears became greatest—the perceived sound was located at *br* 80°. Experimentally it seems to be the fact that when the relative difference is greatest the perceived sound is in general located somewhat to the rear of the visual right and left line.

Similar results were obtained when the experiment was conducted by placing telephones on the right side of the observer at 30° both in front and to the rear. Table XXIII gives the average results.

Table XXIII.

Distance of the secondary coil for the back telephone.	Judgment of direction.	Judgment of distance.
10cm	*fr* 45°	10cm (30cm)
9	*fr* 55	10 (30)
8	*fr* 57.5	9.5 (28)
7	*fr* 65	9.3 (28)
6	*fr* 67.5	9.5 (28)
5	*fr* 66.7, *br* 70 or *fr* 70	7.7 (23), 10 (30)
4	*br* 70, *fr* 75	7.1 (21), 7.5 (22)
3	*fr* 73, *br* 70	6 (18), 5 (15)
2	*br* 72.5	5 (15)
1	*br* 70	4.5 (13)

The number of experiments on each point is 4. The probable error for direction varies from 0 to $\pm 5\frac{7}{10}\%$ and that for distance from 0 to $\pm 10\%$.

Figure 19 shows the results graphically. As the sound heard by the right ear grew stronger the perceived sound was located more towards the side. But as the value of the relative difference between the intensities of the sensations perceived by the two ears was smaller in this case than in the last experiment, the perceived sound was never *l*ocated so near to the auditory axis.

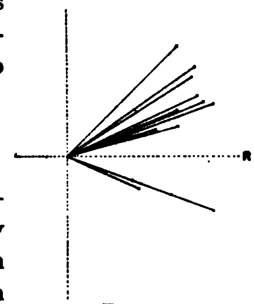

Fig. 19.

V. Perception of distance.

The dependence of the perception of the distance of a sound upon its intensity has already been observed in foregoing experiments, though attention has been paid chiefly to the perception of the direction. In this section particular consideration will be given to the perception of distance with a view to determining under various conditions the relation between the intensity and distance of a perceived sound.

1. *Dependence of the change in the distance of a perceived sound upon the change in the intensity of the component sounds.*

Two telephones were situated on both sides at *r* 90° and *l* 90°. The wires from both telephones were connected with the secondary coil of the sliding inductorium. In this experiment it was requisite to make the intensities of the two component sounds equal in every respect at each distance of the secondary coil. This was done with a fair approximation to correctness. The subject of the experiment was C. W. In this subject the left ear was sharper than the right ear. Taking this fact into consideration we could not expect that the subject would locate all sounds

Table XXIV.

Distance of the secondary coil for both telephones.	Judgment of direction.	Judgment of distance.
9^{cm}	*f*	12^{sun} (36^{cm})
8	*f* (*l* 6.7°)	11.5 (35)
7	*f* (*l* 3.3)	11.3 (34)
6	*f* (*l* 5)	10.7 (32)
5	*f* (*l* 6)	9 (27)
4	*f*	6.7 (20)
3	*f*	8.7 (26)
2	*f*	7.7 (23)
1	*f*	6 . (18)

The probable error for distance varies from 0 to $7\frac{4}{10}$%.

which he perceived strictly in the median plane. It would be more probable that when the intensities of sounds were weakened the observer would project the perceived sound a little towards the left of the median plane.

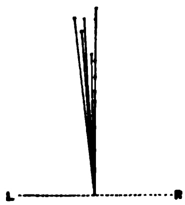

FIG. 20.

A current of $1\frac{1}{2}$ ampères was used in the primary circuit and the sounds were given in an arbitrary order. The average results of three experiments on each point were as given in Table XXIV. Figure 20 shows the results graphically.

When the same experiment was repeated with a current of 2 ampères the average results of three experiments on each point were as given in Table XXV.

TABLE XXV.

Distance of the secondary coil for both telephones.	Judgment of direction.	Judgment of distance.
9^{cm}	f (l 3.3°)	14^{mm} (42^{cm})
8	f (l 3.3)	12 (36)
7	f	10.7 (32)
6	f	9.3 (28)
5	f	9.3 (28)
4	f	7 (21)
3	f	6 (18)
2	f	4.3 (13)
1	f	4 (12)

The probable error for distance varies from 0 to $4\frac{7}{10}\%$.

Figure 21 shows the results graphically. In both experiments the perceived sounds were located in the median plane, though when the sounds grew weaker the effect of the left sound became relatively stronger and the perceived sound tended to be localized a little towards the left of the exact median plane. The distance of the perceived sound gradually increased as the intensity of the component sounds grew gradually weaker.

When the same experiments were repeated and the sounds were given in ascending or descending order the results were more regular, but not much different.

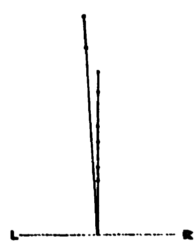

FIG. 21.

The results show that in the case of a familiar sound the judgment of its distance is based upon the difference in intensity.

A fact analogous to the results of these experiments is found in optics where the difference in distance is judged by the apparent magnitude of objects familiar to the sight and of known size.

2. *Relation between the perception of distance of a sound and its intensity.*

In the preceding experiments we have considered the change in the distance of the perceived sound which depends upon the change in the intensity of the telephone sounds owing to the change in the intensity of the electric current. By this method the quantitative relation between the distance of a perceived sound and the physical intensity of the sound cannot be found, for it is very difficult to measure the change in the intensity of a sound, either absolutely or relatively, as depending on the change in the intensity of the electric current. To obtain a quantitative relation I was, therefore, compelled to change the intensity of a sound by changing the distance of the sounding body. By that method the relation between the change in the perception of distance and the change in the intensity of a sound could be more easily established, for the intensity of sound-waves diminishes according to the law of inverse square of distance. I could not perform the experiment for a greater distance than 8 feet, for the cloth chamber within which I was compelled to execute the experiment to avoid reflection of the sound was a cube of 6 feet, the diagonal being about 8.5 feet long. The subject was seated in a chair in one corner of the chamber. A tape measure was stretched from that corner to the opposite corner on a level with the top of the head of the subject; the head was adjusted by a support in such a way that the tape measure would run in the median plane of the head. The point of the measure at which it was intersected by a perpendicular drawn from the middle point of the imaginary line connecting the openings of the ears was regarded as the zero point. I used a telephone sound which was connected with an electro-magnetic fork of 250 vibrations per second. Between the fork and the telephone a short circuit key was inserted, by means of which the duration of the telephone sound could be regulated. The telephone was to be moved below the tape measure and parallel to it, so that it was situated in the median plane at the level of the openings of the ears.

The first step of the experiment was to find the point at which the subject judged the sound to be distant just one foot. Let this point be called *A*. The next step was to give two short sounds with a brief interval between them, the first at the point *A* and the second at a different point. The subject was to judge, with his eyes closed, whether the second sound was twice as far distant as the first sound. If the subject thought that the second sound was nearer or farther than twice the distance of the first sound then the experiment was to be repeated by giving the second sound at a farther or a nearer point. After many experiments a point would be found at which the second sound appeared to be just twice

as far as the first one. In a similar way the points were found, at which the second sound was judged to be three times, four times and five times as distant as the first sounds. In this way the relation between the distances in the mental and physical scales was established. During the experiment the intensity of the telephone sound was kept constant, so that the change in the intensity of the perceived sound would arise only from the change in the distance of the sound, and could be expressed by the accepted law of the propagation of sound.

I made the experiments on the two subjects I. M. and K. M. More than 100 experiments were tried for each point of the scale of distance, care being taken to avoid the effect both of practice and fatigue.

Both subjects judged the sound to be about one foot (3.1^{cm}) distant when the sound was given at the point 40^{cm} distant from the o point of the tape measure. For the subject I. M. the relative mental scale of the distances 1, 2, 3, 4, 5 corresponded to the physical scale of the distances 40, 80.9, 112.4, 159.3, 185.9 cm., and for the subject K. M. the relative mental scale of the distances 1, 2, 3, 4, 5 corresponded to the physical scale of the distances 40, 79.2, 117, 152, 193.5 cm. Now as these physical distances are the distances between the middle point of the auditory axis (connecting the ears) and the telephone, and as the distance between this point and the opening of the ear is about 8^{cm}, we have for the square of the distance between the ear and the telephone the sum of the squares of the above two distances, i. e., the distance between the telephone and the middle point of the auditory axis and the distance between the middle point and the opening of the ear, for these three distances correspond to the three sides as a right-angled triangle. Finding the squares of the distances between the ear and the telephone, which correspond to the distances in the mental scale, we have for I. M., 1664, 6609, 1298, 25440, 34623. Dividing these figures by 1664 (to get the relative distances) we have the ratios 1, 3.3, 7.5, 15.4, 20.8. The squares of the distances for K. M. are 1664, 6337, 13753, 23168, 37506. Finding the ratios we have 1, 3.8, 8.3, 13.9, 22.3.

The ratios in the above two cases are nearly equal to the squares of the distances of the sounds in the mental scale, namely, 1, 2, 3, 4, 5. As the reciprocals of these ratios represent the relative intensities of the sounds, the conclusion appears justified that when the intensity of a sound diminishes in geometrical progression the perceived distance of the sound increases in arithmetical progression.

3. *Continual change in the distance of a perceived sound due to the movement of the two component sounds.*

In connection with the question of distance I conducted other sets of

experiments in which the objective positions of the two component sounds were simultaneously changed, while their intensities were kept constant, and obtained results which showed again the dependence of the perception of distance upon the change in the intensity of the perceived sound. In these experiments the spherical cage (Figure 1) was used.

a. The two telephones were moved along the radii of the horizontal circle of the cage. The directions in which the two telephones were moved are as given below. The arrows indicate the directions, and the letters indicate the points of the spherical cage as explained on p. 3; thus the expression for Number 1 means that the two telephones were placed at first very near to the ears and they were then moved simultaneously to the points *r* and *l*; Number 3 means that they were started at the front and the back very near to the head and were moved to *f* and *b*; Number 5 means that one of the two telephones was started at *r* and moved to the ear, while the other telephone was started at the left ear and moved to *l*; etc.

1. *l* ⟵ ear ear ⟶ *r*; 2. *l* ⟶ ear ear ⟵ *r*;
3. *f* ⟵ head ⟶ *b*; 4. *f* ⟶ head ⟵ *b*;
5. *l* ⟵ ear ear ⟵ *r*; 6. *l* ⟶ ear ear ⟶ *r*.

Five or eight experiments were made for each case and the judgments of the observer T. N. in regard to the directions of the perceived sounds under the conditions were as given below; *n* means the nose, *x* means doubtful, *k* means within the head.

1. *k–f, k–x, k–b(x), k–b(x), k, k(f), k–f and l* ⟵ *k* ⟶ *r, k–f and l* ⟵ *k* ⟶ *r*;
2. *f, b–k, b–k, b–k, b–k, b–k, f–n, k(f)*;
3. *k–f, n–f, n–f, k(f)–f, n–f*;
4. *f–n, f–n, f–n, f–n, f–n, f–n*;
5. *l–r, l–r, l–r, l–r*;
6. *r–l, r–l, r–l, r–l, r–l.*

In Number 1 the two ears of the observer were stimulated at first by strong sounds and he felt the sound in the interior of his head. As the telephones were moved along the auditory axis farther and farther from the ears the intensities of two sounds grew weaker and the perceived sound emerged from the head and receded along the median plane more and more towards *f*. But it receded occasionally towards *b*. This result shows that the continual change in the distance of the preceived sound arises from the continual change in the intensity of the sound. In this experiment the observer perceived sometimes two sounds separately, though at the same time he perceived a third sound in in the median plane. Number 2 is just the reverse of Number 1. The sound was

perceived at first in front or back at a certain distance and then it gradually approached towards the head and at last entered the head.

It is interesting to note here that in Number 2 the sound seemed to start generally at *b* while in Number 1 it seemed to stop generally at *f*, though the intensity of the stimulation of the ears at the instant of starting in Number 2 was just like the intensity of the stimulation of the ears at the instant of stopping in Number 1. In Number 1 the observer was sure that the sound was in front, for in the first half of the experiment the perceived sound was very strong, and therefore he continued to think that the sound was still moving in the same direction even when its intensity grew weaker and weaker. He thought only occasionally that the sound moved towards the back, but he was very doubtful of his judgment. In Number 2 the case is a little different. In this case the sound was heard with less intensity at the first moment than in the succeeding moments, and the observer was in doubt at first whether it was in front or in back. It was his usual experience that when a sound was at the back he was generally in a state of doubt. So in this case he judged that the sound started from the back, and consequently he continued to think that the sonnd was still moving from back even when the sound grew stronger. Thus 1 and 2 show that the perception at a certain instant is influenced by the perception of the preceding instant.

In Number 3 the sound in front was heard with greater intensity than the sound at the back at the same distance. Moreover the sound was heard at first with great intensity. The observer could not, therefore, doubt that the sound was in the front very near to the head. As the intensity became less the distance of the perceived sound increased. Number 4 is the reverse of Number 3 and needs no explanation.

Most interesting results were given by Number 5 and Number 6, in which we found striking examples of the dependence of the perception of distance upon intensity. In these cases the two telephones were moved in the same direction and not in the opposite direction as in the previous four cases. Here the observer perceived the sound to be travelling in a direction reverse to the direction of the movement of the telephones. In Number 5 one telephone was moved from *r* to the right ear, while the other telephone moved from the left ear to *l*, so that the direction of movement of the two telephones was from right to left. The observer perceived the sound to be travelling from left to right. The explanation is simple and clear. At first the sensation of the left ear was relatively stronger, and gradually grew weaker, whereas the sensation of the right ear was at first relatively weaker and gradually grew stronger. At a certain point the intensities of the two sensations became equal. Con-

sequently the sound was at first perceived on the left side near to the ear, then it was perceived in the median plane, and at last it was perceived on the right side near to the ear. The intermediate points were passed in succession. As a whole the sound seemed to have travelled from left to right. Number 6 is just the reverse of Number 5.

All the six cases show that the distance of the perceived sound depends upon the intensity of the sound. The change in the former is continuous when the change in the latter is continuous. Continuous change in the intensity of a sound is always perceived as a change in the distance of the sound, i. e., as a motion of the sound, whether that change in intensity is caused by actual motion or not.

b. The two telephones were moved along the circumference of the horizontal or frontal circle of the spherical cage. The objective path along which the two telephones were moved were as indicated below. The expression for Number 1 means that the two telephones were started at the front and were moved around simultaneously to the left and right; Number 3 means that they were started at the left and right and were moved simultaneously toward the front, etc.

1, $l \longleftarrow f \longrightarrow r$; 2, $l \longleftarrow b \longrightarrow r$; 3, $l \longrightarrow f \longleftarrow r$; 4, $l \longrightarrow b \longleftarrow r$; 5, $l \longleftarrow o \longrightarrow r$; 6, $l \longleftarrow u \longrightarrow r$; 7, $l \longrightarrow o \longleftarrow r$; 8, $l \longrightarrow u \longleftarrow r$.

The number of experiments for each case was from 4 to 6. The judgments of the observer T. N. in regard to the directions of the perceived sounds under these conditions were as follows:

1. *f–n, f–n, f–n, f, f–fo–o.*
2. *b–k, b–k, b–f, b–bo– o* or *f–n.*
3. *k–f, f, k*(*b*) or *k*(*f*)*–f, b–x–f.*
4. *k–b, k–b, k–b, n–b.*
5. *o–k, o–k, o* or *f, o–b.*
6. *u–n, u–k, u–uf–f, u–uf–k.*
7. *n–f, f–b, f–o, f–o, k–o*(*b*)*, f–fo–o.*
8. *n–u, n–u, n–u, k–u.*

In all cases the intensities of the sounds for the two ears were equal and the perceived sound was always located somewhere in the median plane. Special results were observed as follows.

Number 1. When the telephones were moved from *f* to *r* and *l*, the sound was perceived to travel in the median plane from some distant point in front inward to the nose or the head of the observer. This decrease of distance presumably arose from the increase of the intensity of the perceived sound, for under these conditions the perceived sound is stronger on account of the action of the pinnæ, when the objective sounds are situated more towards the auditory axis than when they are situated

more towards front. But as the tragus has some influence upon the intensity of the perceived sound, the observer was sometimes in doubt about the direction of motion of the sound as the telephones were passing about the points *fl* and *fr*.

Number 2. When the telephones were moved from *b* to *r* and *l* the sound was perceived to be travelling from *b* to *k* or from *b* to *f*. This needs no explanation, for it is analogous to Number 1.

Number 3 and Number 4 are just the reverse of Number 1 and Number 2 and the sounds were perceived to pass from *k* to *f* and from *k* to *b* respectively.

Number 5 and Number 6 can be explained in a similar way. On account of the pinnæ and the direction of the external meatus, sounds coming from above or below are heard with less intensity than sounds coming from the direction of auditory axis. Accordingly the intensity of the perceived sound increases gradually as the telephones are moved from above or below toward the auditory axis, and the distance of the perceived sound seems to decrease.

Number 7 and Number 8 are the reverse of Number 5 and Number 6.

It is interesting to note that in Number 7 the sound was sometimes perceived to have travelled from *f* to *b*, or *f* to *o*, or *n* to *f*, instead of travelling from *k* to *o*. From this we can see that in discriminating directions in the median plane much depends upon the interpretation of the observer. In number 7 the stimulation of the ears was strongest in the beginning; then grew less and less as the telephones were moved gradually upward. This decrease of the intensity can be interpreted by the observer as the effect of motion of sound from *f* to *b*, or *f* to *o*, or *n* to *f*.

Finally in connection with the question of distance we must call attention to the endocephalic localization which we have already noticed. When the intensity of the two sounds opposite the ears becomes very great the perceived sound, which is localized at first in the median plane, approaches the head and at last enters it. This endocephalic localization is sometimes so strong that the subject cannot get rid of the illusion, though he knows perfectly well that the objective sounds are outside the head. This illusion occurs more strikingly when the telephones are placed against the ears or when conducting tubes are put into the ears. Under such conditions the sound is heard in the head even if the intensity is not very great. An experiment of SCHAEFER[1] is interesting in this connection. In his experiments a telephone was

[1] SCHAEFER, *Zur interaurealen Localisation diotischer Wahrnehmungen*, Zt. f. Psychol. u. Physiol. d. Sinn., 1890 I 300.

brought near to a funnel receiver, which communicated with the two ears by means of a forked tube with arms of equal length. The telephone was connected with the secondary coil of a sliding inductorium. At the start the secondary coil was put at a great distance from the primary coil of the inductorium and then the secondary coil was gradually brought nearer to the primary, during which the change in localization was observed. It was found that the apparent sound approached the head according to the decrease of the distance between the primary and secondary coils, so that the sound finally crept into the head and occupied a position between the ears. If one of the arms was closed the sound would shift to the auditory canal of the opposite ear. If the secondary coil were then moved farther from the primary coil the sound would go out from the auditory canal to the space on the side of that ear. If the pressed tube were then opened, the sound would move to the median plane at some distance from the head. With many persons the sound entered or emerged from the head at the root of the nose according to the increase or decrease of the intensity of the telephone sound.

VI. The least perceptible change in the direction of a sound.

In our previous experiments we found that when two telephones were placed on opposite sides just in the line of two ears and the intensity of one component of the perceived sound was changed while that of the other was kept constant, the perceived sound was located more toward the side on which the objective sound was stronger, and more toward the median plane when the intensities of the two component sounds became more nearly equal. While conducting these experiments I noticed occasionally that, when the relative intensities of the two sounds were in such a relation that the perceived sound was located at *r* 90° or *l* 90°, a small change (1 to 1½ cm. for the secondary coil) in the difference between the relative intensities of two sounds was not usually perceived as a change in the direction of the perceived sound and, in fact, was not perceived at all. But when the intensities of the two component sounds were in such a relation that the perceived sound was located just in front, the change in the difference between the relative intensities corresponding to 0.5^{cm} or 1^{cm} was usually perceived as a change in direction.

The explanation of the difference in the above two cases seems to me to lie in the fundamental fact of sensation as expressed by Weber's law. For the initial difference between the relative intensities of the sounds heard by the two ears was greater in our experiments when the perceived

sound was located more toward the side, and it was smaller when the perceived sound was located more toward front. Consequently a large change would be necessary in the former case if the change is to be perceived.

The object of the next experiment was to make this relation clear. As in my previous experiments, two telephones were placed on opposite sides. The distance between the ear and telephone was 45^{cm}. The intensity of one component sound was kept constant, while that of the other component sound was varied by means of the secondary coil; the observer localized the perceived sound at a certain point as before when the two components were given simultaneously. This point was to be regarded as the initial direction. Then, while the two sounds were given, one of them was to be moved from its original position slowly along the auditory axis till the point was reached where the observer just noticed a change in the initial direction of the perceived sound. The distance traversed by this component sound was measured by a millimeter scale which was fixed along the path of the telephone. This distance is called an increment distance (Δ), for it is a distance which is required for producing the least change in the initial direction of the perceived sound.

The results of the experiments on the relation between the change in the initial direction and the corresponding increment distance were as given in Table XXVI.

TABLE XXVI.

The right component sound constant. Observer: C. W.

M	*D*	*I*	Δ	*E*	*n*
27	0.6 cm	*f*	13.1 cm.	*r*	15
	2	*fr* 30°	15.6	*r*	15
	3	*fr* 70	21.7	*r*	15
	4	*r* 90	26.1	*r*(*b*)	15
	5	*r* 90 (*b*)	27.5	*r*(*b*)	7

M, date in March, 1897.

D, distance of the secondary coil for the left telephone.

I, direction of the perceived sound for the position *D*.

Δ, change in the distance of the left telephone.

E, direction toward which the sound appeared to change.

n, number of experiments.

The probable error of Δ varies from $\pm \frac{6}{10}\%$ to $\pm 1\frac{7}{10}\%$.

In this experiment the intensity of the sound produced by the right telephone (connected with the primary coil of the inductorium) was kept constant while the intensity of the sound produced by the left telephone

(connected with the secondary coil) was varied. When the secondary coil was moved away from the primary to the point 0.6^{cm} and sounds were given from both telephones at the same time the observer located the single resultant sound at *f*. The resultant sound was localized at *fr* 30°, *fr* 70°, *r* 90°, *r* 90° (*b*) respectively as the coils were separated as 2^{cm}, 3^{cm}, 4^{cm}, 5^{cm}.

When the observer located the sound at one of above directions the intensity of the left component was decreased again by moving, not the secondary coil, but the left telephone itself from the ear along the auditory axis, till the point was reached at which the sound was just noticed to shift from its initial position.

As we find in the table the increment distance was least when the perceived sound was initially located at *f*, it increased gradually till it reached its maximum when the perceived sound was initially located at *r* 90°(*b*). When the initial difference between the intensities of components heard by the two ears was much greater the increment distance became greater than 35^{cm}. In our small room we could not make an arrangement to move the telephone farther than 35^{cm}. The reason why the perceived sound shifted its position always toward the right in this experiment is easy to find, for the intensity of the right component became relatively greater as the intensity of the left component decreased. We must notice here the fact that the perceived sound was located at *r* 90°, but a little backward than just at *r* 90°, when the relative difference between the intensities of sounds in the two ears was very great. A similar case has been already observed in our previous experiments. I have said that it is probable that what we call commonly right and left lies in the line drawn tangent to the front surfaces of the two eyeballs. So when the maximum difference in the intensities of the components in the two ears is obtained, the perceived sound is not located just at visual right or left, but, being referred to the common standard of direction, it is located at right or left, a little toward back. Or we may say that the difference in the relative intensities with which a sound is heard by the two ears is greatest, not when the sound is situated in the visual *rl* line, but when it is situated a little behind it. On this basis the fact that the sound seemed to shift a trifle backwards from *r* 90° when the intensity of the right component became relatively very strong does not disprove the dependence of the least perceptible change in the direction of a sound upon the change in the relative difference between the intensities of the components in the two ears. From similar experiments conducted on a different day, results were obtained in which the above relation can be seen still more clearly (Table XXVII).

Owing to the slight changes in the position of the telephones and the head of the observer and minute changes in the intensity of the sound arising from the fluctuaton of the electric current, there were some deviations in conditions for different days; and consequently the results of the experiments must be considered for each day separately.

TABLE XXVII.

The right component sound constant. Observer: C. W.

M	*D*	*I*	*Δ*	*E*	*n*
24	1 cm	*l* 90°	24.9 cm	*f*	15
	2	*bl* 70	17.9	*b*	15
	4	*f*	12.5	*r*	15
	4.5	*b*	15.7	*r*	15
	6	*fr* 60	14.2	*r*	15
	7	*r* 90	25.7	*r*(*b*)	15

Notation same as in Table XXVI.

The probable error of Δ varies from $\pm 1\frac{4}{10}\%$ to $\pm 2\%$.

Some experiments made upon Dr. Scripture gave similar results which are given in Table XXVIII.

TABLE XXVIII.

The right component sound constant. Observer, Dr. Scripture.

M	*D*	*I*	*Δ*	*E*	*n*
27	0.6 cm.	*fl* 10°	23.3 cm.	*f*	9
	3	*f*	20.7	*r*	10
	5	*fr* 60	22.7	*r*	10

Notation same as in Table XXVI.

The probable error of Δ varies from $\pm 2\frac{5}{10}\%$ to $\pm 3\frac{8}{10}\%$.

In these experiments the perceived sounds were sometimes located to the rear. In such cases the least change in direction was also made toward *b*. Again, when the initial direction was on the left side the least perceptible change was made toward the median plane. The shifting of of the left sound toward the median plane is really a shifting toward the right side, and can be explained by the relative increase of the intensity of the right component sound.

Similar experiments were made upon C. W. on different days, by varying the intensity of the right component sound while keeping that of the left component sound constant, and similar results were obtained, which are given in Table XXIX and need no special explanation.

TABLE XXIX.

The left component sound constant. Observer: C. W.

M	*D*	*I*	*Δ*	*E*	*n*
18	3^{cm}	*r* 90	29.1^{cm}	*l*	14
	5	*fr* 70	26.4	*l*	18
	8.25	*f*	15.1	*l*	15
19	10	*l* 90	24.6	*l*(*b*)	20
	7	*fl* 70	22.4	*l*	20
	6	*f*	20.5	*l*	19
20	2	*br* 80	34.7	*b*	5
	4	*fr* 20	24.4	*f*	12
	7	*l* 90	33.2	*l*(*b*)	4
31	11	*fl* 80	33.6	*l*(*b*)	15
	10	*fl* 20	21.4	*l*	17
	9	*f*	17.6	*l*	15
	7	*fr* 10	25.6	*l*	13
	5	*fr* 20	31.7	*l*	15

D, distance of the secondary coil for the right telephone.

Other notations same as in Table XXVI. The probable error of *Δ* varies from $\pm \frac{4}{10}\%$ to $\pm 2\frac{6}{10}\%$.

As the intensity of sound is inversely proportional to the square of distance, the intensity of the variable component sound in the foregoing experiments is proportional, other things being equal, to $\frac{1}{(C+\Delta)^2}$ where C represents the initial distance (45^{cm}) between the telephone and the ear and Δ represents the increment distance. As C is constant in our experiments, the value of the fraction depends upon Δ. The greater the change in the increment distance, the greater is the change in the intensity of the component. But the change in the increment distance which is required for producing the least noticeable change in the initial direction would be expected according to WEBER's law to depend upon the value of the initial difference between the intensities of the two components. This expectation was realized in the actual results of the foregoing experiments. For in these experiments the increment distance was smallest when the sound was initially located in front or behind, that is, when the initial difference between the intensities of the component sounds in the two ears had the smallest value possible (namely, zero). The increment distance increased gradually as the initial difference between the intensities of the component sounds in the two ears became greater, and consequently as the perceived sound was initially located more towards the right or left side.

VII. THEORETICAL CONCLUSIONS.

Sounds are located both as to direction and distance from ourselves as the center. In the foregoing experiments we have found that the judgment

of the direction of a sound depends chiefly on the relative difference between the intensities of the component sounds heard by the two ears, while the absolute intensity of the perceived sound is supplementary to this fundamental factor. The judgment of the distance of a sound depends, on the contrary, chiefly on the absolute intensity. To these factors in the localization of a sound we may add other coöperating factors, such as relative and absolute pitch, timber and phase.[1] With these data at hand it becomes necessary to inquire how they bring about the localization of a sound. There are several theories of the way in which this is done; these will be briefly discussed.

1. *Theory of a direct acoustic space.*

This theory assumes that a sound heard by the right ear is distinguishable from that heard by the left ear; that the right and left components of a sound heard with both ears produce an effect in consciousness which varies with the relative intensity of the components; that this effect is an experience analogous to that from simultaneous but not identical sensations of sight or touch; that it should be considered to be one of the kinds of space analogous to visual space or tactual space; and that this acoustic space is brought into relation to and modified by visual, tactual and muscular experiences.

This might be called a *direct* theory of acoustic space. Although no positive objection can be made, it seems somewhat artificial and not well adapted to explain the methods by which we localize sounds.

2. *Tactual theory.*

Some psychologists have sought the ultimate origin of acoustic space in the tactual sensations of the tympanic membrane.

One form of this theory assumes: 1, that special sensations of touch are received from different parts of the tympanum; 2, that the sound-wave arouses the sensations from the tympanum; and 3, that different

[1] URBANTSCHITSCH, *Zur Lehre von der Schallempfindung*, Archiv f. d. ges. Physiol. (Pflüger), 1881 XXIV 574.

RAYLEIGH, *Our perception of the direction of a source of sound*, Nature, 1876 XIV 32. RAYLEIGH, *Acoustical observations*, Phil. Mag., 1877 (5) III 456.

MACH, *Bemerkung über die Function der Ohrmuschel*, Arch. f. Ohrenheilk., 1875 n. F. III 72.

THOMPSON, *On binaural audition*, Phil. Mag., 1877 (5) IV 274; 1878 VI 383; 1881 XII 351. THOMPSON, *On the function of the two ears in the perception of space*, Phil. Mag., 1882 (5) XIII 406.

BLOCH, *Das binaurale Hören*, Zt. f. Ohrenheilk., 1893 XXIV 25.

parts of the tympanum are affected according as the direction of the sound is different.[1]

The first assumption is in all probability justified; the tympanum is undoubtedly sensitive and the stimuli applied to different portions can be presumably distinguished.

The second assumption is not so readily to be accepted in view of the fact that the energy of the air-vibrations is extremely small, in fact so small that a most complicated apparatus, the internal ear, has been specially adapted to transform the air-vibrations into nerve-currents. There is no conclusive experimental evidence to support the view that sound-waves can be directly felt by the end organs or the nerves of touch. In the perception of sounds the tympanum is not the true receptive organ, but only a part of a mechanical device by which the vibrations are gathered and conveyed to the internal ear.[2]

The third assumption is quite untenable. The diameter of the auditory canal is too small to permit of different degrees of pressure at different points of the tympanum at the same moment; we can without hesitation consider the pressure as equal over all parts of the tympanum.[3]

A second form of this theory to the effect that the touch organs of the tympanic membranes in the two ears are affected differently by sounds coming from different directions would be consistent with observations of cases in which the ability to localize sounds was lost, together with the sensitiveness of the tympanum.[4] In spite of some minor observations that may be interpreted in favor of this theory it is difficult to agree with WUNDT[5] in accepting it even partially; at any rate such an extremely remarkable sensitiveness of the tympanum should be most thoroughly established before a final decision could be given in favor of it.

3. *Theory of a special space organ.*

According to PREYER the semicircular canals are the organs for the perception of the direction of a sound.[6] By means of a specific energy of the

[1] KUPPER, *Ueber die Bedeutung der Ohrmuschel des Menschen*, Archiv f. Ohrenheilk., 1874 n. F. II (3) 158.

[2] THOMPSON, *On the function of two ears in the perception of space*, Phil. Mag., 1882 (5) XIII 406.

[3] HELMHOLTZ, *Die Mechanik der Gehörknöchelchen und des Trommelfells*, Archiv. f. d. ges. Physiol. (Pflüger), 1868 I 1.

[4] GELLÉ, *Rôle de la sensibilité du tympan dans l'orientation au bruit*, Soc. de Biol., 1886 III 448.

[5] WUNDT, Physiol. Psychol., II 94, Leipzig, 1893.

[6] PREYER, *Die Wahrnehmung der Schallrichtung mittelst der Bogengänge*, Archiv. f. d. ges. Physiol. (Pflüger), 1887 XL 586.

ampullæ of the semicircular canals a particular kind of sensation is produced when they are excited by a sound from a particular direction. The sensation is different according to the direction from which the sound comes, because a sound must stimulate more strongly one canal or pair of canals in a way depending on its direction. As the six canals are excited with different relations of intensities by sounds from different directions, the sensations which are produced by the specific energy of the ampullæ are different corresponding to the different directions of the sounds. These differences of sensations give us the ideas of the directions of sounds. "If we consider," says PREYER, "that by the stimulation of an ampulla with the vibration of the liquid of the canal belonging to that ampulla a sensation of sound is produced which is different from the sensation produced by the stimulation of another ampulla, though the two are exactly alike in respect to intensity, pitch and timbre, and if the difference between the two arises simply because the different nerve-fibers are stimulated, we cannot but regard this difference as spatial."

PREYER's assumption that a canal is most strongly stimulated by a sound lying in the plane in which the canal lies is irreconcilable with the elementary facts of the physics of sound. The sound-waves which are transmitted through the air reach the membrana tympani and then move the chain of ossicles, and at last the lymphatic liquid of the inner ear. During this conduction the sound-wave is changed into a movement of a membrane, a movement of a set of bones, a movement of a liquid, etc. There is no possibility of any change of direction of these movements for changes in the direction of the sounding body, and consequently no possibility of a varied action on the semicircular canals.

PREYER may have thought that the sound-waves are conducted directly from the air through the skull bones into the head and thus to the semicircular canals; at least, this seems the only possible construction of his view. The conduction of sound-waves by the cranial bones comes into play when a sonorous solid body is either in immediate contact with the skull, or is connected with it by a chain of solid or fluid bodies, or when the medium immediately surrounding the head is not gaseous as, for instance, when the head is immersed in water. With aquatic vertebrates we can not deny the cranial conduction of sounds, but for most sounds it is certain that in the case of human beings the cranial conduction plays no part. The sound waves are mostly reflected from the surface of the head, and the conduction directly through the cranial bones and tissues is infinitesimal.

PREYER's theory next involves the assumption that the sound-waves

affect the canals differently, according to the direction of the sound. He evidently viewed the sound as a force entering the head and being resolved into three components with intensities depending upon the angles made by the direction of the force with the planes; he had probably in mind the familiar method of resolving a movement or a velocity into three components in planes at right angles to each other. The absurdity of such a position is at once evident to any one acquainted with the elementary ideas of wave-motion. A sound-wave passing through the air consists of alternate condensations and rarefactions—not of a line or of a plane—but of a more or less spherical surface. The sound-wave passing through the head is of a similar sort. There is not the remotest reason for believing that the molecules of a liquid in a curved tubular bone will vibrate differently according as the sound-wave approaches from different directions in the mass in which the bone is imbedded.

This theory of PREYER's was accepted by MÜNSTERBERG[1] as the starting point of a reflex-muscular theory of acoustic space. "The different movements of the head, which can be aroused by stimulation of the semicircular canals, arouses—by means of the muscle sense—that threefold system of sensations of movement which forms the basis of our acoustic space." "To localize a sound means to assign to its place in the whole system of sensations of head-movements the sensation of that reflex head-movement which is necessary in order to turn toward the source of the sound."

Concerning the possibility of the direct stimulation of the semicircular canals nothing further needs be said.

As for the results of MÜNSTERBERG's experiments on the least perceptible change in the direction of a sound, they cannot be regarded as a proof of his theory. It has been made clear by the experiments of BLOCH[2] and by mine that the curve of the least perceptible change can be explained by the principles of relative and absolute intensities.

4. *Motor theory.*

The essential factor in the motor theory seems to be this: A sound perceived by the ear brings with it an impulse—conscious or unconscious—to move toward it; this motor impulse and its results are what appear to us as the localization of the sound. This theory assumes that these motor impulses are aroused in definite relations by the factors men-

[1] MÜNSTERBERG, *Raumsinn des Ohres*, Beiträge z. exper. Psychol., 1889 II 182.
[2] BLOCH, *Das binaurale Hören*, Zt. f. Ohrenheilk., 1893 XXIV 25.

tioned at the beginning of this section (p. 70), namely, absolute intensity of the sound, relative intensity for the two ears, etc. The form of the motor space is derived from past experience under influence of the visual space; in fact, the space in which the sound is localized is our usual visual-tactual-motor space with which the sound is connected by the motor impulses. In this compound space the visual sensation is the most important element. The tactual sensation seems rather to be of secondary importance except in the case of the blind. Speaking genetically we can recognize the position of a sound only when acoustic and visual sensations are connected with each other in a definite relation. It is unthinkable that we can recognize the position of a sound without connecting the former directly or indirectly with the latter. A definite connection between these two can be accomplished through the medium of a definite muscular action. These three elements have occurred in connection with each other, and have been firmly associated in the course of time, so that when one of them is present the others will be necessarily called forth. When the visual sensation fails, as in the case of a blind person, then tactual sensation takes its place.

From a biological point of view this theory seems quite natural. In the course of natural selection the survivors have obtained the power of reacting suitably to the different acoustic sensations which they receive from the surrounding world.

To react suitably upon an acoustic sensation we must first of all recognize the direction from which the sound comes. To recognize the direction it is necessary that the auditory sense should be assisted by other senses. But as the auditory sense has to do with more or less distant object, it must be chiefly the visual sensation which is combined with the auditory sensation to assist it in perceiving the direction of the sonorous object. To perceive an object in a certain direction the head must be moved so that the object will be brought in the line of visual fixation or in the median plane. In the beginning this will not be easily done, but after practice it will be found that the instant at which the sound is equally loud in both ears is the instant at which the source of sound is found in the median plane. Again we have already seen that a sound situated in the median plane will be best heard when it is in the line of sight. Thus after the source of sound has been brought into the median plane, the next process will be to bring it into the line of sight.

Again we must notice the fact that by practice it has been found that the instant at which the sound is perceived with greatest intensity by one ear and with smallest intensity by the other ear is the instant at which the source of sound is situated nearly in the auditory axis or little

behind it. Accordingly, we sometimes try, by the movement of the head, to bring a source of sound into this axis when we find it more convenient, as for a sound of small intensity.

These movements of the head, which we perform in order to connect visual and acoustic sensations, affect our sense of equilibrium, by means of which we become conscious how much we have turned our head. By means of this sense of equilibrium we are enabled to estimate the position of a source of sound in reference to the position of our body.

Again, though a source of sound is usually brought into the line of fixation of sight by the motion of the head, we can effect this within a certain limit not by moving the head but by moving the eyeballs alone. In such a case the acoustic sensation is connected with the oculo-motor sensation, by means of which we can feel also the angular direction of a source of sound, for we have a fine sense for this movement of the eyes. This muscular sensation of the eyes is supplementary to the sensation of rotation of the head, by means of which we can chiefly estimate the direction of a sound.

From these considerations it is clear that by long practice the association has been established between a particular acoustic sensation—corresponding to the stimulation by the sound from a particular direction—and the rotatory sensation—which is required for bringing the source of sound into the visual fixation line or into the auditory axis—and moreover the association has been established between the acoustic sensation and the oculo-motor sensation. After the associations between these factors have been established, either one of these factors by itself is able to call forth other factors. It is not necessary that the rotatory sensation or muscular sensation, which gives us the measure of the angular departure of a source of sound from the fixation line of the eyes, should arise always by an actual movement. Though this is the original case, the sensation of innervation or reproduced motor sensation called forth by association with the acoustic sensation may take the place of the actual motor sensation.

Our final conclusion is thus that an acoustic sensation receives its spatial form primarily from the space idea which is given to us by the visual, tactual and motor sensations. Acoustic space presupposes the existence of the space form of other sensations. We have only to give an account of how the perception of the position of sounds arises on the basis of the already existing space which was given to us by other sensations. As to the further problem of the ultimate origin of the space form of perception, its solution must be sought in the visual and tactual perception.

ON BINAURAL SPACE.

BY

E. W. SCRIPTURE.

The fundamental fact of binaural space is that when two component sounds heard by the two ears are perceived as a single sound the resultant sound is "localized in space." This "localization in space" is a direct experience of every person with binaural audition.

Aside from such problems as the influence of visual and muscular space on this localization, the fundamental problem is that of the localization of the resultant sound as dependent on the two components. The simplest sounds, tones, vary in pitch, intensity and duration. The only property which comes essentially into question in the case of the two components is that of intensity. The following paragraphs will present a hypothesis in respect to the law of localization as dependent on the intensity of the components.

The fundamental observation is as follows: When two component tones, e. g., from the telephones opposite the two ears, are heard as one tone, this tone is located in a certain direction in respect to the observer. When the intensities of the two components are equal, the resultant appears to be in the median plane, e. g., directly in front. As one of the components is weakened, the resultant appears to pass toward the side of the stronger one, finally reaching a line nearly opposite the ear—the auditory axis—and proceeding outward along it. The localization thus depends on the difference between the intensities of the components. Various observations lead me to believe that the following equations express this dependence.

Let I_R and I_L be the intensities of the right and left components, and let $d = I_R - I_L$ be the difference between the two intensities.

Let the plane in which the resultant lies contain a system of rectangular coördinates, with the origin in the median plane, the axis X identical with the acoustic axis, and the axis Y perpendicular to X; thus Y may lie anywhere in the median plane. Since the position of the sound with respect to these axes depends on the difference of intensity, we have $x = f(d)$ and, since there is a definite relation between y and x, $y = f(x)$.

In the experiments it is observed that when the two sounds are equal, i. e., $d = 0$, the resultant is located in the median plane, i. e., on the

axis Y at a certain distance m from the center. Thus for $d = 0$ we have $x = 0$, $y = m$. As the sound on the right is made louder we have $I_R > I_L$ and d positive; when the sound on the left is made louder d becomes negative. As one component becomes louder than the other, the resultant moves toward the side of the louder component; indicating the right side by $+$ and the left by $-$ we have $+x = f(+d)$ and $-x = f(-a)$. The resultant lies always on the positive side of the Y axis, which we can express by considering y as a function of the square of x, or $y = F(x^2)$.

The path described by the resultant sound appears to me to be a curve of the form given by the equation

$$y = me^{-\frac{x^2}{am}}$$

where m is the value for $x = 0$ and a is a constant of proportionality.

On the basis of these observations and considerations I venture to make the following two hypotheses: 1. that the distance right or left of the median plane is proportional to the difference between the intensities of the two components, i. e., $x = cd$, when c is the factor of proportionality; 2. that the relation between the distance from the median plane and the distance from the auditory axis is expressed by

$$y = me^{-\frac{x^2}{am}}$$

where m is the distance of the sound when $x = 0$ (i. e., $d = 0$), and a is a proportionality factor.

The values m and a depend on certain properties of the sounds used, but mainly on the absolute intensity. Sometimes the sound appears to remain always in the auditory axis, in which case $m = 0$. A series of curves for different values of m is shown in Fig. 22. The values from which the curves were plotted are given in the table.

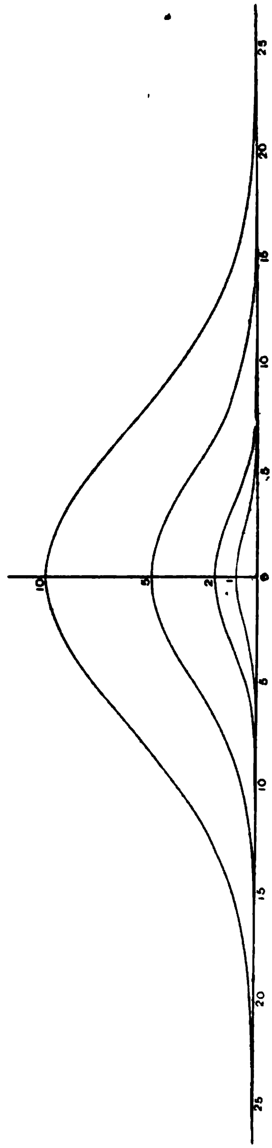

FIG. 22.

TABLE OF $y = me^{-\frac{x^2}{am}}$ FOR $a = 10$.

x	$m = 10$: $p_{10} = \frac{x^2}{100}$	$r_{10} = p_{10} \times 0.4343$	$1.0000 - r_{10} = \log y_{10}$	y_{10}	$m = 5$: $p_5 = \frac{x^2}{50}$	$r_5 = p_5 \times 0.4343$	$0.6990 - r_5 = \log y_5$	y_5	$m = 2$: $p_2 = \frac{x^2}{20}$	$r_2 = p_2 \times 0.4343$	$0.3010 - r_2 = \log y_2$	y_2	$m = 1$: $p_1 = \frac{x^2}{10}$	$r_1 = p_1 \times 0.4343$	$0.0000 - r_1 = \log y_1$	y_1
0	0.00	0.0000	1.0000	10.000	0.00	0.0000	0.6990	5.000	0.00	0.0000	0.3010	2.000	0.0	0.0000	0.0000	1.000
1	0.01	0.0043	0.9957	9.902	0.02	0.0087	0.6903	4.901	0.05	0.0217	0.2793	1.904	0.1	0.0434	0.9566—1	0.905
2	0.04	0.0174	0.9826	9.607	0.08	0.0347	0.6643	4.616	0.20	0.0869	0.2141	1.637	0.4	0.1737	0.8263—1	0.670
3	0.09	0.0393	0.9607	9.135	0.18	0.0785	0.6205	4.174	0.45	0.1964	0.1046	1.273	0.9	0.3927	0.6073—1	0.405
4	0.16	0.0695	0.9305	8.521	0.32	0.1389	0.5660	3.681	0.80	0.3480	0.9530—1	0.897	1.6	0.6949	0.3050—1	0.202
5	0.25	0.1086	0.8914	7.787	0.50	0.2172	0.4718	2.956	1.25	0.5430	0.7580—1	0.573	2.5	1.0857	0.9141—2	0.082
6	0.36	0.1564	0.8435	6.974	0.72	0.3127	0.3763	2.379	1.80	0.7817	0.5193—1	0.331	3.6	1.5635	0.4365—2	0.037
7	0.49	0.2128	0.7872	6.126	0.98	0.4256	0.2734	1.878	2.45	1.0640	0.2370—1	0.173	4.9	2.1280	0.8720—3	0.007
8	0.64	0.2779	0.7221	5.273	1.48	0.5559	0.1431	1.391	3.20	1.3897	0.9113—2	0.082				
9	0.81	0.351	0.6482	4.447	1.62	0.7036	0.9954—1	0.989	4.05	1.7589	0.5421—2	0.035				
10	1.00	0.4343	0.5657	3.679	2.00	0.8686	0.8305—1	0.677	5.00	2.1715	0.1295—2	0.013				
11	1.21	0.5255	0.4744	2.981	2.42	1.0509	0.6481—1	0.445	6.05	2.6275	0.6735—3	0.005				
12	1.44	0.6254	0.3746	2.369	2.88	1.2508	0.4492—1	0.281								
13	1.69	0.7340	0.2660	1.845	3.78	1.4679	0.2310—1	0.170								
14	1.96	0.8512	0.1488	1.409	3.92	1.7026	0.9964—2	0.099								
15	2.25	0.9771	0.0229	1.069	4.50	1.9543	0.7447—2	0.056								
16	2.56	1.1112	0.8888—1	0.774	5.12	2.2236	0.4754—2	0.030								
17	2.89	1.2550	0.7440—1	0.554	5.78	2.5100	0.1890—2	0.015								
18	3.24	1.4071	0.5919—1	0.391	6.48	2.8142	0.8848—3	0.009								
19	3.61	1.5778	0.4222—1	0.264												
20	4.00	1.7372	0.2628—1	0.183												
21	4.41	1.9153	0.0847—1	0.122												
22	4.84	2.1020	0.8980—2	0.079												
23	5.29	2.2974	0.7026—2	0.050												
24	5.76	2.5016	0.4984—2	0.032												
25	6.25	2.7144	0.2856—2	0.019												
26	6.76	2.9359	0.0641—2	0.012												
27	7.29	3.1650	0.8350—3	0.007												

There still remains the fact that the plane of XY (which we have considered) may be in any position around the auditory axis; thus the sound may pass in front of, above, behind or below the head, or in any intermediate position. Four such positions are shown in Fig. 23. To fully

FIG. 23.

define the apparent position of the sound, we must introduce a system of coördinates in which X is the auditory axial line through the head, Z is a line perpendicular to this at the central point in the head and extending in the direction which the subject considers to be directly in front, and Y is perpendicular to both X and Z. We thus have $x = cd$ as before. Then in a case where the sound lies in the XZ plane to the front we have $y = 0$ and

$$z = me^{-\frac{x^2}{am}}.$$

When the sound is upward in the XY plane we have $x = 0$ and

$$y \; me = {}^{-\frac{z^2}{am}}.$$

The complete relation is expressed by

$$x = cd,$$

$$y = me^{-\frac{x^2}{am}} \cdot \sin a,$$

$$z = me^{-\frac{x^2}{am}} \cdot \cos a,$$

where a is the angle of elevation above the plane XZ.

This series of hypotheses agrees with the facts reported by MR. MATSUMOTO in the preceding pages, but cannot be proven until the experiments are repeated with tones of carefully measured intensities. I cannot say that I expect them to be confirmed just as they stand; I propose them simply as an attempt to give definite form to our notions of one of the laws of binaural localization.

THE SIZE-WEIGHT ILLUSION AMONG THE BLIND.

BY

JAMES F. RICE, M.A.

It is a well known fact that when two objects of equal weight but different size are lifted, the smaller appears heavier than the larger. The phenomenon has been made the subject of experiment in various ways.[1]

It was suggested to me that some experiments on the blind might be of interest. The experiments were performed at the New York Institution for the Blind. They were carried out under the direction of the Yale laboratory with the suggestion-blocks formerly used by Dr. SEASHORE. Many suggestions in regard to the details of the experiments were received from Dr. SEASHORE personally.

APPARATUS.

The apparatus consisted of two sets of cylindrical blocks 31^{mm} in length. Each set consisted of 17 blocks. Set *A* varied in size and had a uniform weight, while Set *B* varied in weight and had a uniform size. The blocks in Set *A* varied in diameter according to a geometric series in which the regular increment is one-tenth. Those in Set *B* were arranged in arithmetic series according to weight with a successive difference of 5^g.

In the following account the blocks will be dintisguished by the names *A* and *B* with their respective numbers in the series.

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[1] FECHNER, *Ueber die Contrastempfindung*, Ber. d. k.-sächs. Ges. d. Wiss., math. phys. Cl., 1860 XII 76.

MUELLER and SCHUMANN, *Ueber die psychologischen Grundlagen der Vergleichung gehobener Gewichte*, Archiv. f. d. ges. Physiol. (Pflüger), 1889 XLV 37.

CHARPENTIER, *Analyse de quelques éléments de la sensation de poids*, Archives de Physiol., 1891 (5) III 122.

DRESSLAR, *Studies in the psychology of touch*, Am. Jour. Psych., 1894 VI 313.

FLOURNOY, *De l'influence de la perception visuelle des corps sur leur poids apparent*, L'Année Psychol., 1894 I 198.

GILBERT, *Researches on the mental and physical development of school-children*, Stud. Yale Psych. Lab., 1894 II 43–45, 59–63.

PHILIPPE and CLAVIÈRE, *Sur une illusion musculaire*, Revue Philos., 1894 XL 674.

VAN BIERVLIET, *La mesure des illusions de poids*, L'Année Psychol., 1895 II 79.

GRIFFING, *On the sensations of pressure and impact*, Psychol. Rev., 1895 II Suppl. I.

SCRIPTURE, *Remarks on Dr. Gilbert's article*, Stud. Yale Psych. Lab., 1894 II 102.

The blocks of Set *A* were of a constant weight, 80^{g}, and of diameters in millimeters as follows, beginning with the smallest: 20.0, 22.0, 24.2, 26.6, 29.3, 32.2, 35.4, 39.0, 42.9, 47.2, 51.9. 57.1, 62.8, 69.1, 76.0, 83.6, 91.9.

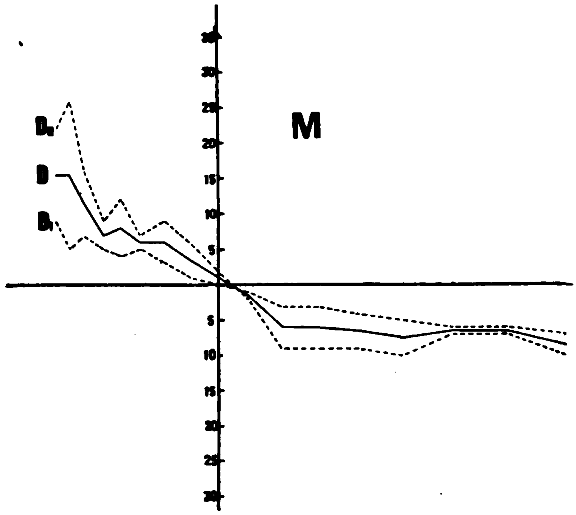

FIG. 24.

The blocks of Set *B* were of a constant diameter, 42.9^{mm}, and of weights in grams as follows, beginning with the lightest: 40, 45, 50, 55, 60, 65, 70, 75, 80, 85, 90, 95, 100, 105, 110, 115, 120.

It is to be observed that the uniform weight for Set *A* is the same as the weight of *B* (9), the middle block in Set *B*; and the uniform size in Set *B* is the size of *A* (9), the middle block in Set *A*.[1]

The observer placed himself by the table, on which the blocks were arranged in order, in such a position that by moving back and forth he could lift any block from its place in Set *B* and still retain approximately the same angle of the arm and hand. He was requested to select for each block in Set *A* a corresponding one in Set *B*, by taking one at a time from *A* and placing it by the side of successive blocks in *B* with which he wished to compare it, lifting one at a time until he found the one in *B* which he thought had the same weight as the one from *A*.

One series of tests *D* was made in which the size of the weights was learned by grasping the curved surface of the block; a second series of tests *E* was so made that the observer could only judge of the size from

[1]SEASHORE, as before; SCRIPTURE, New Psychology, Fig. 65, London 1897.

the area of pressure when the block was placed gently upon the palm of his hand. All the *A* blocks were used in each series. Each series of tests was made ten times, and, to eliminate as far as possible the error of prejudice, the equivalent *B* for each *A* block was approached five times from above, five times from below. That is, in five of each series an *A* block was compared first with a *B* that was very perceptibly heavier and then with the *B* of next lower weight until apparent equality was reached. In the other five the steps were from the perceptibly lighter.

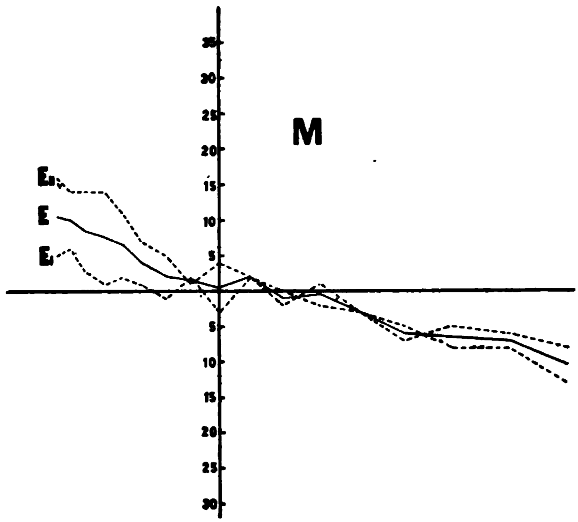

FIG. 25.

To eliminate the error of sequence, the observer lifted the blocks in the orders *A*, *B*, *B*, *A*, and *B*, *A*, *A*, *B* alternately. The position of the blocks was reversed after each trial, so that the observer's judgment was not affected by the varying sensations caused by the slight movement of the forearm to the right or left. By this exchange of position there was also to a slight extent avoided the fixing of the observer's attention upon the *A* with which the several *B*'s were being compared; his judgment was, without explicit reference to either block as a standard, merely a judgment of equality. In case of a perceived difference he indicated which was the heavier.

SUBJECTS.

It is desirable for experiments on the psychology of the blind that the subject should have been totally blind from birth. It has been held, in-

deed, that with the congenitally blind we should class as competent observers those who became blind during their first year,[1] and many who have studied the blind have included in their observations those who had lost their sight as late as the seventh year.[2] Obviously the mental life of those whose experience includes light sensations cannot be identical with that of those totally blind from birth. The assumption that the conditions are similar cannot be established until the psychology of the congenitally blind, who have never seen light, has been first studied. The subjects in the experiments here described have been blind from birth and have never seen light.

M is a college graduate and university professor; he is a mathematician of international reputation. *O*, his brother, is in business in New York. They are both graduates of the New York Institution for the Blind.

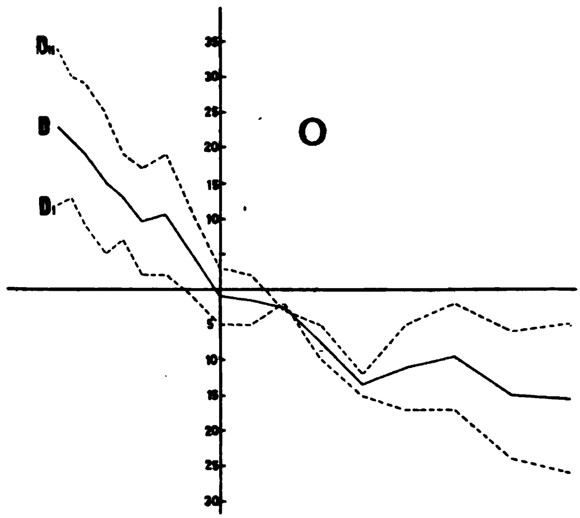

FIG. 26.

M was from the start quite aware of the illusion, and though he was kept in ignorance of the purpose of the tests, he repeatedly spoke of the impossibility of correct estimates of weight when the objects compared were of different sizes. It was his opinion that of the two tests the second, i. e., with the weights on the palm of the hand, was the least accurate. His custom had always been to compare weights by grasping in quick

[1] HELLER, *Studien zur Blinden-Psychologie*, Phil. Stud., 1895 XI 252.

[2] HOCHEISEN, *Ueber den Muskelsin bei Blinden*, Zt. f. Psych. u. Phys. d. Sinn., 1893 V 239.

succession the objects to be judged, and dropping them, if small, from one hand to the other, or by weighing them upon the tips of the fingers, as in the case of coins. In the methods of these experiments he had no previous experience.

O was also fully aware of the illusion. He, like *M*, considered the third series of judgments the least satisfactory, and expressed the same

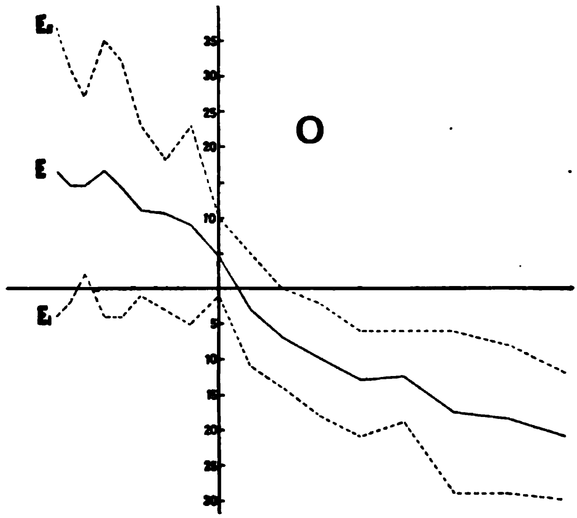

FIG. 27.

preference for grasping rather than mere lifting upon the palm. *O* thought that size was less diverting in the first series than in the second.

EXPERIMENTS.

(1) The first series of tests was that in which the knowledge of size was gained through the muscle sense, corresponding to SEASHORE'S "fourth series, H, muscle sense." An *A* and a *B* block, resting on end upon a soft pad, were lifted in succession, being grasped around the circumference by the thumb and middle finger of the right hand. If the weights were judged unequal, the *B* was replaced by another of the same set. Because the observer did not have to make any choice of *B*'s, but to consider only two blocks at a time (selected by the one conducting the experiment), he could fix his attention upon the question of equality of weight undistracted by the knowledge of the number of blocks that might possibly be compared.

(2) In another set of experiments the block was laid on the palm of

hand ; this gave an idea of the size, the height being known to be constant. This corresponds to SEASHORE's "fourth series, I, touch."

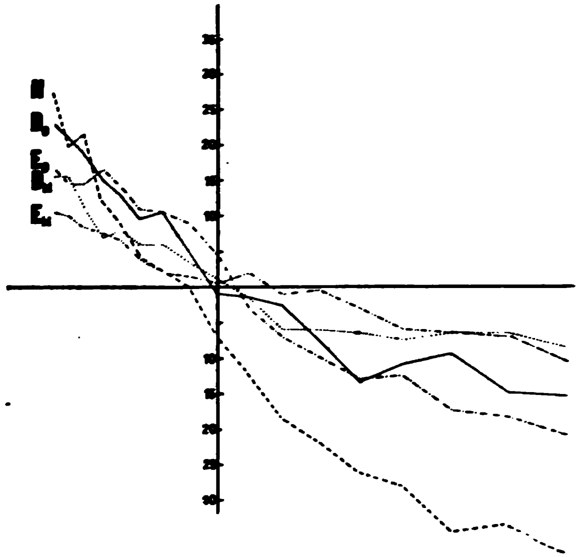

FIG. 28.

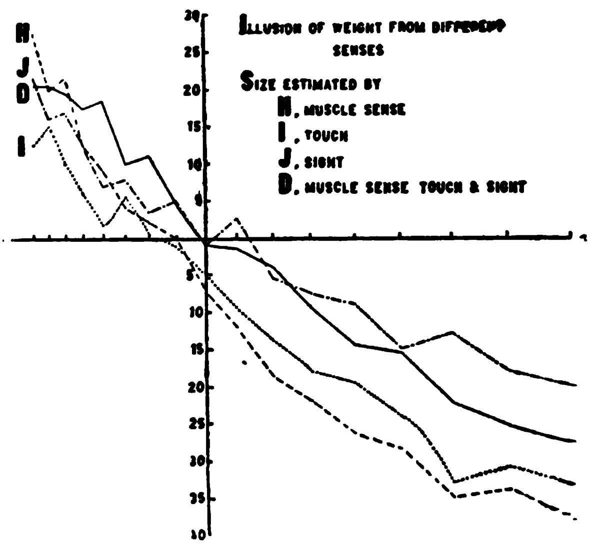

FIG. 29.

The results are given in the table. Each result in the columns D_I and D_{II} is the average of 5 original measurements; the probable error for these results never exceeds 4% and is generally less than 2%. The results in the table are indicated graphically in Figures 24, 25, 26, 27. Figure 28 shows the average results for both observers together with the curve H of Dr. SEASHORE's experiments.

	M						*O*					
C	D_I	D_{II}	D	E_I	E_{II}	E	D_I	D_{II}	D	E_I	E_{II}	E
−22.9	+9	+22	+16	+5	+16	+11	+12	+34	+23	−4	+37	+17
−20.9	+5	+26	+16	+6	+14	+10	+13	+30	+22	−2	+31	+15
−18.7	+7	+16	+12	+3	+14	+9	+9	+29	+19	+2	+27	+15
−16.3	+5	+9	+7	+1	+14	+8	+5	+25	+15	−4	+35	+16
−13.6	+4	+12	+8	+2	+11	+7	+7	+19	+13	−4	+32	+14
−10.7	+5	+7	+6	+1	+7	+4	+2	+17	+10	−1	+23	+11
−7.5	+3	+9	+6	−1	+5	+2	+2	+19	+11	−3	+18	+8
−3.9	+1	+6	+4	+2	+1	+2	−1	+11	+5	−5	+23	+9
0	0	+2	+1	−3	+4	+1	−5	+3	−1	−1	+10	+5
+4.3	−1	−2	−1	+2	+2	+2	−5	+2	−1	−11	+5	−3
+9.0	−3	−9	−6	0	−2	−1	−2	−3	−5	−14	0	−7
+14.2	−3	−9	−6	−2	+1	0	−10	−5	−7	−18	−2	−10
+19.9	−4	−9	−6	−3	−3	−3	−15	−12	−13	−21	−6	−13
+26.2	−5	−10	−7	−5	−7	−6	−17	−5	−11	−19	−6	−12
+33.1	−6	−7	−6	−8	−5	−6	−17	−2	−9	−29	−6	−17
+40.7	−6	−7	−6	−8	−6	−7	−24	−6	−15	−29	−8	−18
+49.0	−7	−10	−8	−13	−8	−10	−26	−5	−15	−30	−12	−21

M, *O*, subjects.

C, number of millimeters by which the diameter in Set *A* differed from that in Set *B* (having a weight of 80g).

D_I, D_{II}, D, number of grams by which the estimated weight of the block in Set *A* differed from its true weight, the block being grasped.

E_I, E_{II}, E, same as D_I, D_{II}, D, the block resting on the skin.

D_I, E_I, experiment begun with the lighter blocks.

D_{II}, E_{II}, experiment begun with the heavier blocks.

D, E, averages of D_I and D_{II}, and of E_I and E_{II} respectively.

For the sake of comparison the curves obtained by SEASHORE for the same senses in the seeing are reproduced in Fig. 29 from Fig. 5 of SEASHORE's monograph.

CONCLUSIONS.

The results seem to justify the following conclusions:

1. The size-weight illusion obtains among the blind.

2. It follows the same general law as among the seeing, but is not so great either for the muscle sense or for touch as for the same senses among the seeing.

3. The illusion tends towards the side on which the weight is approached.

CEREBRAL LIGHT.[1]

BY

E. W. SCRIPTURE.

In darkness or with closed eyes we can always see irregular forms of light in our visual field. These forms are of various kinds, series of waves, successive rings that spread and break, etc. In addition to these definite figures there is always more or less definite irregular illumination over the whole field. These phenomena are generally called "the retinal light" or the "*Eigenlicht* of the retina." They are usually supposed to arise from chemical changes going on in the retina. I wish to record some observations that apparently prove them to be cerebral and not retinal processes.

1. With closed eyes there is only one illuminated field, not two, as there should be from the two retinas if the light were retinal. Two retinal figures might appear as one under the conditions: (*a*) of suppression of one field, which is not the case here, because it is impossible to keep one field suppressed for many minutes, whereas I have watched the retinal figures in uninterrupted continuance for a long time; (*b*) of perfect identity of form, which is hardly a possible supposition in the case of these irregular, volatile, chemical phenomena; (*c*) of sufficiently similar construction for union by stereoscopic vision, which also is not the case, as there is no relief-effect in the picture.

2. The figures do not change in position when the eye is moved. They are localized in front and remain in the same place, even if the eyes are directed to one side. I find, however, that if the eyes are turned to a new position and kept there, the central figure (a spreading violet circle with a phosphorescent rim) will soon afterwards follow the movement; there is thus a tendency for this figure to occupy the spot of sharpest vision.

3. The figures do not change in location when the eyes are displaced. When the eyes are looking at some definite object, e. g., this page, a pressure of the finger on one of them will cause the page apparently to move. This is true whether the other eye is open or closed. Likewise, if an after-image is obtained, it will move upon pressure of the eyeball. The pressure displaces the eyeball and changes the projection of the re-

[1] This account was first published in Science, 1897 N. S. VI 138.

tinal picture. This displacement does not occur with "retinal light." I have repeatedly observed these figures and have manipulated the eyeballs; I have found that they are not in the slightest degree affected by the manipulations. In order to avoid all possibility of errors of observation, I have made the experiments in a series alternately with eyes open and eyes closed. With the eyes open I observed a dimly illuminated window; with them closed I saw the "retinal" figures. The former always followed the displacements, the latter never.

These observations are, I believe, sufficient to establish the proposition (which I have not seen elsewhere) that the phenomena of vision usually known as "retinal light" and "retinal figures" are not originated in the retina, but in the brain. They should, therefore, be termed "cerebral light" and "cerebral figures."

The following hypothesis seems also justified. The cerebral light is located in those higher centers of the brain which are connected with visual memories and imaginations. While watching the cerebral figures I find that my visual memories or phantastic figures appear in the midst of the cerebral light and frequently cannot be distinguished from them. The close connection of these cerebral figures with the contents of dreams has been repeatedly noticed by JOHANNES MUELLER and a series of later observers. There is also the possibility that the hallucinatory visions produced by hashish, mescal and other drugs may be simply modifications of this cerebral light.

RESEARCHES ON MEMORY FOR ARM-MOVEMENTS.

BY

E. W. SCRIPTURE, W. C. COOKE AND C. M. WARREN.

In the arm-space board[1] a wooden scale carries along its upper edge, a small glass rod. At the zero point in the middle there is a fixed metal plate. On each side there is a movable slide carrying an adjustable pointer. Before the experiments the pointers are pushed forward as far as possible.

The apparatus is placed on a table with the scale away from the subject. The subject, seated with eyes closed or covered, places his forefingers against the zero-plate, one on each side.

The experimenter moves up the two slides to the fingers till they press gently. The pointers strike the zero-plate and are pushed back automatically. This eliminates the errors due to the widths of the finger, as all readings are to be taken from the end of the pointer.

The subject places himself directly in front of the zero-mark and closes his eyes. The experimenter places the left-hand (referring to the subject) slide at a certain distance, d. The right-hand slide is moved out of the way. The subject moves his left fore-finger evenly outward till it strikes the slide, and then returns it to zero. The experimenter quietly moves the slide out of the way, and after an interval of t seconds the subject moves his finger again till it seems to be in the same place as before. The experimenter now moves the slide up till it touches the finger and reads the record at the end of the pointer. The tenths of a centimeter are estimated by the eye. The result in millimeters is placed in the record blank.

In a set of experiments carried out by MR. COOKE in 1896–97 on four college students as subjects, the distances 100^{mm}, 300^{mm} and 500^{mm} were investigated for the intervals 2^s, 10^s and 20^s. The constant and probable errors were calculated in the following way. If $a_1, a_2, \cdots, a_n$ are the observations for given values of t and d, we have for the average

$$a = \frac{a_1 + a_2 + \cdots + a_n}{n},$$

from which we obtain the constant error $C = a - d$.

[1] SCRIPTURE, *Elementary course in psychological measurements*, Stud. Yale Psych. Lab., 1896 IV 97; SCRIPTURE, New Psychology, 187, London 1897.

For the variations, or errors, we have $v = a_1 - a$, $v_2 = a_2 = a, \cdots$, $v_n = a_n - a$; from which we obtain the probable error for a single measurement by the shorter method

$$P = 0.8 \frac{|v_1| + |v_2| + \cdots + |v_n|}{n - 1}$$

where $|v|$ means that the sign of v is disregarded. The results for four subjects, A, B, C and D, are given in Table I; five experiments were made on each point.

TABLE I.

Distance.	Time.	A *C*	A *P*	B *C*	B *P*	C *C*	C *P*	D *C*	D *P*
100 mm.	2s	+ 11	5.8	+ 11	8.9	+ 2	3.0	− 2	8.9
	10	+ 22	12.1	+ 10	8.9	− 10	6.5	− 4	7.1
	20	+ 15	12.5	+ 15	8.9	− 16	2.4	− 4	6.4
300 mm.	2s	+ 6	12.8	+ 10	8.9	− 5	11.2	+ 9	8.9
	10	+ 6	24.6	− 13	11.9	− 30	13.6	− 8	16.9
	20	− 5	14.5	− 13	13.0	− 27	17.6	− 12	24.0
500 mm.	2s	+ 6	7.4	+ 7	8.3	− 7	12.9	− 5	7.8
	10	− 2	8.1	− 8	7.5	− 1	9.4	− 15	14.8
	20	− 1	7.3	− 5	7.5	+ 6	11.0	− 24	21.7

The table seems to justify the conclusion that the law according to which the constant error varies in relation to the elapsed interval, is a purely individual matter, as was first pointed out by SCRIPTURE (New Psychology, 189).

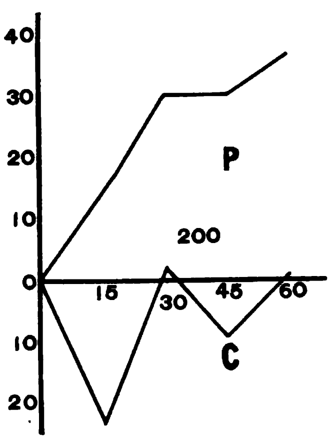

FIG. 30.

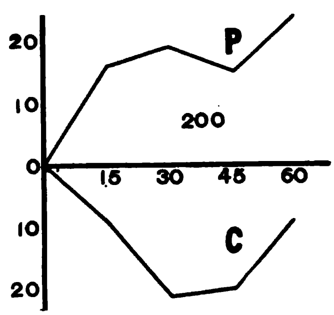

FIG. 31.

The probable error also seems to follow no general law, although there are more cases in which it increases with the increase in interval than in which it decreases or remains constant or fluctuates.

For comparison with these experiments on untrained observers another set was undertaken by Mr. WARREN in March, 1898, in which the subject was the laboratory janitor, A. FISHER, a trained observer[1] with no interest in the results. The constant error was calculated as before, but the probable error was derived by the more accurate formula

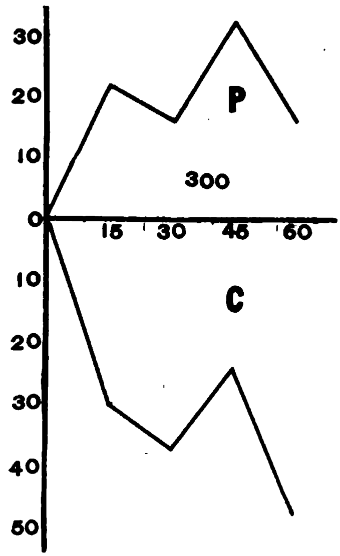

FIG. 32.

$$P = \tfrac{2}{3}\sqrt{\frac{v_1^2 + v_2^2 + \cdots + v_n^2}{n-1}}.$$

The results for the intervals 15^s, 30^s, 45^s and 60^s and for the two distances 200^{mm} and 300^{mm} are given in Table II. The first division for 200^{mm} and that for 300^{mm} give the results when the experiments were made in the following order: 15^s — 200, 300, 300, 200; 30^s — 200, 300, 300, 200, etc. The other experiments on 200^{mm} were made on successive occasions, the distance being constant but the intervals of time being varied irregularly, thus 200^{mm} — 15, 60, 45, 30, etc. The values of the table are expressed in Figures 30, 31 and 32.

TABLE II.

	200^{mm}			200^{mm}			300^{mm}		
Time.	*C*	*P*	*n*	*C*	*P*	*n*	*C*	*P*	*n*
15^s	—23	17.3	17	— 9	16.3	15	—30	21.8	17
30	+ 2	30.2	17	—21	18.5	15	—37	15.9	17
45	— 9	29.5	12	—20	15.1	15	—24	31.5	12
60	+ 1	37.2	12	— 9	23.6	15	—47	15.7	12

These experiments seem to justify the conclusion that even for the same person the law of the constant error changes with the distance and with the method of the experiment. The probable error, however, is quite evidently an increasing function of the time-interval; the experiments are, however, not extensive enough to justify an attempt at determining its law.

[1] Stud. Yale Psych. Lab., 1896 IV 24.

PRINCIPLES OF LABORATORY ECONOMY.

BY

E. W. SCRIPTURE.

One of the most difficult portions of the work of the experimental psychologist is the development of the technical methods required by the science. One set of these methods comprises those involved in managing a laboratory. The following results of an experience of four years as a student in the laboratories of Leipzig and Worcester and of six years as director of the Yale laboratory may be of value to others. The material comprises the substance of what I am accustomed to say on this subject in a few lectures of the regular technical course for specialists in psychology.

GENERAL PLAN.

Two distinct kinds of work fall upon the laboratory; they may with fairness be characterized as: 1, college work; 2, university work.

College work.

In a college the aim is to provide an outline-knowledge of the subject sufficient for general culture. Research or advanced courses are quite out of the question.

In a small college the resources must necessarily be limited. With an appropriation of a few hundred dollars the instructor can get along with one or two rooms, an equipment of tables and chairs, apparatus for time-records, sight, hearing, etc. What can be done on the subject of the senses at small expense is shown in SANFORD'S Course in Experimental Psychology, Part I, Boston 1898. However, it must be borne in mind that the book covers only a portion of the science and it is very important that the instructer should not give the impression that this is all of psychology. In a laboratory with moderate means the mechanical skill and ingenuity of the instructor come especially into play. With a fair supply of tools the trained man can give a more interesting and valuable course at a small expense than an unskilled man with far greater facilities. The frequent inquiries concerning the amount requisite for starting a college laboratory may be met with the statement that it all depends on the man employed. Success with only $300 or $500 a year is possible for a skillful man who has had a thorough training, and

who has had experience as an assistant in a larger laboratory. With $500 at the start, about $200 should be put aside for running expenses and for unforeseen contingencies. For $50 a good lathe and a number of small tools can be obtained. If the electric current is available in the building, a hand-feed arc lamp can be imported for about $5, a resistance coil for $2.50, a pair of condensing lenses for $1.50, a simple objective for $1; the rest of the lantern can be made as indicated in these Studies, 1896 IV 84. If electricity is not available, a different luminant may be used. An English oil-lantern need not be expensive; an acetylene lantern is not excessively costly and is very successful; an oxy-hydrogen lantern for compressed gases, though more costly, is very satisfactory, especially if fitted with the LINNEMANN burner for zircon-plates. The slides can be made by the instructor or by an interested student; a very large number of experiments can be performed with the lantern instead of with "demonstration" apparatus. For a few dollars each the following articles may be obtained: color wheel, Kolbe's color cylinders, color discs, Bradley's pseudoptics, stereoscopes, stereoscopic diagrams, etc. More costly but necessary pieces are: recording drum, tuning fork, telegraph keys, batteries, etc.

For the larger colleges a more elaborate equipment is necessary. Comparisons are constantly drawn between the various departments, and merely as a matter of self-preservation the psychological laboratory must offer courses equal in attractiveness and value to those of physics, chemistry and biology. A lecture-room with at least a single lantern and a satisfactory equipment of demonstration apparatus should be provided. The elementary laboratory course should be followed by a carefully planned course in psychological measurements. Part of the course in measurements given at Yale is printed in these Studies, 1896 IV 89–139. The appropriation and yearly income of the laboratory must be quite considerable, if the lectures are to be on an impressive scale and the two laboratory courses are to be really valuable.

University work.

The university student chooses to study psychology either because he will need it in connection with philosophy, pedagogy or medicine, or because he expects to be a psychologist himself. The laboratory should furnish instruction for both classes of students.

In the first place a general lecture-course should be provided with full demonstrations of all the most important methods and results in all the fields of psychology. The apparatus required must necessarily be of a distinctly high grade. The university student demands the best in-

struction. Moreover, if the other departments, such as physics can show better, brighter and more numerous pieces of apparatus the students are apt to draw disparaging conclusions. The students are no longer a "class" to be taught; they are an "audience" that must be led. When a speaker, whether on account of himself or on account of meager equipment, loses the interest or the respect of his audience, the instruction ceases to be effective.

Laboratory courses, like those already mentioned, should also be given. For the special students in psychology a course of lectures on the theory of statistics and measurements should also be given in connection with practical exercises in the laboratory. These exercises should be most carefully planned to show the development of the final methods of measurement from the fundamental operations. For example, the subject of time might begin with timing and regulating a clock; thereupon would follow the timing of a tuning fork, the determination of the errors of markers, keys, etc. When this has been learned simple reactions can be measured under the various conditions of stimulation, attention, etc. Finally, the more complex processes are to be recorded. Similar methods could be followed for sight, hearing, volition, emotion, etc.

The object of the laboratory instruction is often misunderstood. It is not to teach facts of psychology; these should be treated in a lecture-course. The objects are: (1) the cultivation of the powers of introspective and external observation; (2) the development of dexterity in carrying out psychological experiments and measurements; (3) training in the computation and adjustment of measurements. Students who make a specialty of psychology should also be trained in the construction and care of apparatus, in machine-design, use of tools, lathe work, vise work, etc.

Another department of university work lies in research. The art of research is probably the most difficult thing to teach. Some persons have the notion that anybody can successfully undertake researches with no preliminary training. Research is an *art*, as much an art as painting or singing. Any one can daub paints, sing tunes and investigate the mind, but the results differ from a painting by Correggio, a song by Patti, or an investigation by Helmholtz. The director of a laboratory for research must be a man with an inborn tact at overcoming obstacles of apparatus and in extricating himself from intricacies of method. He must be a man of vivid imagination; just as a painter must compose and constantly modify the ideal picture in his mind's eye, so the scientist must outline, invent and modify mentally the whole apparatus and method employed before he begins the actual work. Just as a painter must acquaint him-

self with the minutest details of preparing and mixing paints, of perspective, shading, varnishing, etc., so must the psychological investigator have in mind just as many kinds and combinations of apparatus, manipulations and methods as he can possibly learn. This is the ideal which the special student should always have in mind. The young investigator should go through a preliminary apprenticeship in assisting more advanced workers, and should prove himself a well qualified man before attempting to conduct work.

Building.

If possible the laboratory should be situated in a building back from the street, preferably in the suburbs. If it is in a building with other departments it should have the top floor, and the smaller rooms for observation should be at the back. Just as freedom from shaking is the indispensable condition for many physical experiments, so freedom from noise is the fundamental requisite for the successful prosecution of many psychological investigations. Of course, where only demonstrational work is required such conditions are unattainable and unnecessary.

In regard to the general plan of a scientific building reference may be made to Durm's Handbuch der Architektur, IV. Theil, 6. Halb-Band, 2. Heft: *Gebäude für Erziehung, Wissenschaft und Kunst.*

Lecture-room.

One of the first cares of the instructor will probably be the lecture-room. Although one like that to be described will seldom be attainable, the main requirements are the same for most cases and they can be met in ways as nearly like the ideal ones as possible. All seats in the room should command a good view of the experiment table. In a small institution a large table in an ordinary lecture-room would be sufficient. With large classes, however, the seats must rise toward the back; the rise should be about 12^{cm} at the second row with a regularly increasing rise for each succeeding row. If the seats are arranged in curves, a rather flat hyperbola ought to be chosen. Before the windows black shades or shutters for darkening should be placed. The many elaborate arrangements in use are generally very costly; recourse is generally had to common spring rollers. For further suggestions see Weinhold's Physikalische Demonstrationen and Frick-Lehmann's Physikalische Technik.

The ceiling and walls of the room should be as white as possible. The researches in school-hygiene have shown that dark colors, panelings, rows of blackboards and any other arrangement that lessens the diffusion

of light must be most scrupulously avoided.[1] The daylight should come from the left and back. Artificial illumination should be indirect or semi-indirect; that is, the light from the luminous body should not fall directly on the books of the students, but should be diffused by being first sent against the ceiling or by passing through translucent globes.[2]

The method of completely indirect illumination can be perhaps best used in the case of an arc light. The reflector beneath the lamp hides it completely from view and sends the light to the ceiling, whence it is dispersed around the room. The ceiling should be newly whitened each year; a special reflector, however, may be used above the lamp instead of the ceiling. In the combined method of illumination part of the light passes directly through a lower globe of translucent glass while part is reflected upward. For lecture-rooms the completely indirect illumination seems preferable.

Other sources of light, such as the AUER gas incandescent lamp, the electric incandescent lamp, etc., may be treated in like manner. Further details and literature may be found in the article by BAYR. A good account of arc-lighting is given in a pamphlet, *Ueber Bogenlampen für zerstreutes Licht*, published by the manufacturers, KÖRTING & MATHIESEN in Leutzsch, near Leipzig.

A fundamental piece of apparatus for the lecture-room is the lantern. A general sketch of the principles involved has been given in these Studies, 1896 IV 82–88; further details may be found in WRIGHT's Optical Projection.

Another fundamental factor of the lecture-room is the experimenter's table. This should be a table, or rather a counter, about $5 \times 0.9 \times 0.9$ meters in size. Better than one table are two tables, each two meters long, placed endwise with an intervening space of about a meter which can be covered by a hinged leaf; this allows very tall apparatus to be set up on the floor between other apparatus. The space under the table is utilized for drawers and open closets where tools and apparatus of constant use are kept.

The fundamental rule in regard to the use of the table is: never allow any projection to be fastened to the top. Nails must not be driven in; they are sure to be forgotten and to get in the way at some critical moment, perhaps to overturn or scratch some valuable piece of apparatus. All nails and screws should be driven into blocks of wood which are fastened to the edge of the table by clamps. This rules applies to all

[1] BURNHAM, *Outlines of school hygiene*, Pedagog. Seminary, 1892 II 9.

[2] BAYR, *Ueber Beleuchtungsversuche, etc.*, Zt. f. Schulgesundheitspflege, 1898 XI 129.

tables in the laboratory except when, after trial, it is decided to set up some permanent piece of apparatus.

WORKSHOP.

As the progress of psychology depends to a great degree on the way in which this department is managed, money invested in a proper equipment of tools will often bring better returns than if spent in ordering instruments from dealers. In fitting up this room we should take into consideration its purpose and that becomes clear when we take note of a few facts about apparatus. Apparatus in use industrially, such as electric motors, telegraph keys, batteries, etc., can be obtained readily and cheaply in America. On the contrary, apparatus that is in use only for scientific purposes can be obtaind here only at exhorbitant prices if at all, and owing to the lack of trained workmen it is generally of the poorest quality. All such instruments already in use, e. g., kymographs, tuning-forks, time-markers, etc., must be gotten directly from the European makers. In addition to these two classes there is apparatus for special purposes which must either be ordered from instrument makers or made in the laboratory workshop. The difficulty and delay in sending designs to Germany or France and the great disadvantage of not being able to supervise the various stages of construction are readily apparent. It is highly desirable in a large laboratory to set up a workshop and to provide a proper artisan to take charge of it. The work to be done will be the making of all special research-apparatus, also the repairing and testing of all instruments received from elsewhere. This course is the one followed by nearly all European laboratories of physics and physiology ; the high wages and lack of skill of the American mechanic tend to throw the psychologist more on his own resources.

The size and equipment of such a workshop depend on the character of the instructor and the importance of the part played by research.

The first necessity of the workshop is sufficient light, both by day and by night ; that is, there must be sufficient windows and lamps so arranged that there are no deep shadows into which the work in hand can happen to get. Nevertheless, no very bright or blinding lights are to be permitted. The room is preferably placed on the north side, otherwise the south windows must be well provided with curtains. The lamps are not to be placed in the middle of the room, but to be scattered over the walls and around the tables.

The most important and essential article in the workroom is the work-bench, which should be placed on the lightest side of the room, preferably in the corner. It is to be a very strong table of oak, or ash, which is

fastened by braces built into the wall, so that in fili ing in the vise no noticeable shaking of the ber should be about $2^m \times 0.7^m$; the height should be regular mechanic's height, 0.9^m, is to be reduced bec tinual standing, which is a great hardship to persor such as will necessarily often be at work at the to be placed at the right front corner of the table a site the middle of the window. A smaller vise r left-hand side of the table. Next in importance to lathe. It is to be placed before the window on the n There should also be a grindstone, with pulley in In one corner there may be a forge beside which i oaken block. By the eastern wall of the room ther ter's bench unless a separate room is to be given ov Around the walls are the tool cases. The tools sible, be put in straps or leaned up against groo order on the shelves of the cases. The cases and are to be painted with a light color for sake of illu is to be varnished; this protects it against too rap cleaning, and avoids the odors that arise from the unvarnished floors. A stone floor is inadmissible. gard to the workshop may be found in LEHMANN'S P

ELECTRICAL CONNECTIONS.

An essential for first-class work in psychology is person experimented upon from all sources of distur involves the separation of the experimenter from t on and often involves the use of two or more sep which the only connection is by means of electric w

The experience of telegraphy and telephony has method of connection is by means of wires to a cen point involved. Wires should be brought from ea board in some convenient place where they can be desired. A switchboard on the plan of a telepho 56 terminals is used at Yale; each wire is joine numbered. By means of flexible connectors any can be made and yet any unused wire left unt should be done strictly in accordance with the reg of underwriters, a copy of which can be obtained insurance office. It is not advisable to use wire of 2.7^{mm} (No. 12, American gauge).

Collection of apparatus.

On account of moisture and dust the apparatus should not be kept in rooms frequently used. It is best to devote a special room to this purpose. The floor should be of wood and the cleaning should be done with a moist cloth. The cases for holding the instruments are to be furnished with glass doors which should be kept tightly closed. Each piece of apparatus should have a name or a number and a specified place in the case or cases; small gummed labels or jeweller's tags are useful for this purpose. Small cards with the names of the pieces are fastened on the shelves of the cases by card-pins.

Each piece of apparatus should be entered under its name in the inventory and the apparatus book. The latter should contain for each piece a complete record of its purchase, its use, its constants, where it is to be sent for repair, where to obtain parts, where to find literature on its use, etc. For this purpose a regular letter file can be used; all the data—bills, printed descriptions, etc.—can be filed with the rest of the account.

Isolated room.

To obtain the quiet necessary for most psychological investigations an isolated room may be provided, where the person experimented upon can be kept indefinitely in perfect external darkness and quiet. A description of the Yale room and suggestions for improvement may be found in these Studies, 1893 I 271, 1896 IV 16, and in Scripture's New Psychology, 136, London 1897.

Other rooms.

The other rooms will vary so much in arrangement with the construction of the laboratory that little can be said definitely. I would suggest: 1. an optical room with black walls, heliostat window and photometer arrangements; 2. a time room with clock, chronograph, chronoscope and other equipment; 3. a photographic room.

Selection of apparatus.

Let us suppose that the appropriation has been obtained and that the director has, after carefully planning an ideal laboratory, made up his mind to adjust himself to the actual situation. The next thing to do is to select apparatus. For this purpose he should obtain catalogues from the chief makers and also lists of the apparatus possessed by other laboratories. The catalogues should be carefully studied. The director must at any time know just where to obtain each piece at the lowest price for the best quality. For example, electrical tuning-forks will

be described and illustrated in 15 or 20 catalogues, whereas there are not more than two places from which they can be bought to the best advantage. The list of needed apparatus is finally made out; when added up, it will be found to be four or five times the entire appropriation. A process of selection must now be instituted. I venture to propose a rule, almost self-evidently proper, yet often neglected: select those pieces that for each dollar of cost will bring a maximum return in time of use and in results obtained. For example, let us suppose it necessary to choose between the ELLIS harmonical ($65), a color-apparatus for excentric parts of the retina ($15), and a kymograph ($175).

It would hardly be justifiable to occupy more than half an hour of a lecture-course in experiments for which the harmonical can be used. Let us suppose that the class includes forty persons. The time-value of the harmonical can then be said to be $\frac{1}{2} \times 40 = 20$ efficient hours per year. Let the deterioration and interest on investment on all the instruments be placed at 10% per year. The cost of the harmonical is thus $6.50 for 20 hours, or about $0.33 per hour per student. The color apparatus would be used for about the same time, giving a cost of $0.08 per hour per student. The kymograph is, as we all know, in constant demand; in a busy laboratory where research is going on it will probably be used not less than 2 hours per day for 35 weeks of the academical year, or over 400 hours for research. For lectures on time, memory, motion, etc., it will be used for a total of, say, 5 hours per year for 40 persons, or 200 hours more. This makes 600 hours per year for $17.50 or about $0.03 per hour. Thus the kymograph is ten times as profitable as the harmonical, and nearly three times as profitable as the color apparatus.

This method can be applied to every piece of apparatus, every tool, every piece of furniture, etc., yielding a definite answer in each case. The trouble, of course, lies in estimating beforehand the amount of use to which an instrument will be put. The success with which this is done is what is known in the commercial world as "business sagacity," "business tact and experience," etc.

Perhaps in this connection it may be desirable to state the results of experience at the Yale laboratory with a few of the more important parts of the equipment. One of the most profitable parts is the lantern equipment, representing an investment of about $400, and used on about 60 occasions per year for periods of 10 to 30 minutes for classes of 60 to 130 students. Fully as profitable is the lathe equipment ($100) with motor ($150), shafting, belting, grindstone and the various accessories. The circular saw is also of creditable efficiency. The extensive equipment of small tools and supplies of all sorts is of special value where apparatus is

made. The total workshop equipment is worth about $400 exclusive of the constantly replenished supplies; it is in use eight hours a day by the mechanic and approximately four hours a day by workers in the laboratory. When we consider that much of our best research work and many of our demonstrations would be impossible without the aid of the workshop, we are forced to conclude that this equipment is very profitable in spite of the large expense for wages and materials. Moreover, in a new science where new pieces of apparatus are required for every important advance, the cheapest way of getting such apparatus is by making it in the laboratory. In regard to apparatus such high returns for investments are not to be expected; our most profitable pieces have been the recording drum for hand or motor, 100 v. d. electric fork, spark coil, multiple key, chronoscope, kymograph, piston recorder, etc. Apparatus used for research should be judged by the value of the results obtained.

In business there are losses as well as profits; in managing a laboratory we cannot always make the most judicious selections. Nevertheless, by applying these principles of laboratory economy we can make the money invested bring much larger returns than by spending it hap-hazard. The difference between an economically managed business and a loosely managed one is the difference between success and failure. Laboratory economy does not, after all, differ fundamentally from business economy. High grade efficiency cannot be attained anywhere unless the man at the head knows his business down to the minutest details. The best laboratory is not the one on which most money has been expended, but is the one that yields the largest net result in scientific research and in instruction for each dollar expended.

Economy in investigation.

In a large laboratory with many investigations going on, it becomes a very difficult task to adjust matters economically. Each investigator thinks only of his own needs and is generally oblivious to the fact that when a piece of apparatus is monopolized by him it is lost to every one else. He is likely, for example, to set up the kymograph in such a way that no one else can use it, whereas with just the same labor it might be arranged for every one. He should be taught to choose and arrange his apparatus so as to produce a minimum disturbance. Suppose that he wishes to produce a click for a warning-signal and that there are two sounders and only one relay in the apparatus case. The relay is generally the easiest to adjust; by taking it, however, he would get no better sound and would render it impossible for any else to use relay currents. The investigator should learn to use the cheapest materials at hand. If

for example, he wishes to conduct a telephone-
follow his first impulse to use very large, silk-cov
cords made for incandescent lamps, but should co
mon office wire.

Still more important is it for the student to
execute his researches. The ignorant method of
measurements in large quantities leads to nothing.
vestigation should be first definitely stated. As s
ciently advanced to involve measurements, the in
clear to himself whether the work is to consist i
single quantity or in a full investigation. In the
before making his final measurements, examine an
sources of error, and should carefully estimate an
obtain the required degree of accuracy. The ins
and for carrying out the measurements are to be
Physikalische Maassbestimmungen, Berlin 1886, V
omitting certain parts that refer to full investigati
full investigation, where the object is the determin
pressing the dependence of one quantity on othe
becomes much more complicated. The student s
the whole first volume of WEINSTEIN. This w
rather voluminous and it contains no psychological
hoped that a smaller work on the subject will be w
use of psychologists, economists and biologists.

Any attempt to carry out serious investigation
of the principles involved results in a wasteful pili
out any adequate return in the way of laws or fa
an extravagant procedure has become impossible i
it must soon become so in experimental psycholog

The proper understanding of methods of measure
requires a familiarity with at least the elements of a
calculus. The student can get along with what is
SCHOENFLIES's Einführung in die mathematische
wissenschaften, or, in regard to calculus, with FISHE
lus, although a more extended study like that invol
such familiar books as STEGEMANN-KIEPERT's Diff
rechnung or SCHLOEMILCH's Compendium der Ana
given by FUHRMANN, Naturwissenschaftliche Anwe
und Integralrechnung, is highly desirable. Wi
mentary acquaintance with calculus and a familiar
the science of measurements higher scientific work
ligible and inaccessible to the student of experime

NOTES.

In reply to requests for the colors of the glasses employed in the color-sight tester described in these Studies, 1895 III 103, the following complete list is given: (1) dark gray; (2) ground glass; (3) medium gray; (*A*) red; (*B*) ground glass; (*C*) blue; (*D*) green; (*E*) green; (*F*) brownish yellow; (*G*) ground glass; (*H*) gray glass; (*I*) green; (*J*) bluish green; (*K*) red; (*L*) violet. This arrangement is made for reasons that will become evident to the users of the tester. When the test is being made, the subject should be made to promptly call off the colors in the order 1, 2, 3. The dichromats of the first-class (SEEBECK), or the so-called green-blind, make mistakes on light green and dark red; those of the second class, or the red blind, make mistakes on the light red and dark green. Both classes almost invariably call the violet "blue." Plain dark grays will be frequently called green by the first class, and red by the second. These results hold good for the color-weak of both classes, provided the instrument is used in a properly moderate light, for example, not in sunlight.

In order to advance psychological work at this period when good special instruments are hardly obtainable, the Director of the Yale Laboratory offers to furnish to experimental psychologists—as far as may be practicable—blue-prints of the working-drawings of any of the special Yale pieces. A nominal charge of ten cents each will be made to cover the cost of paper and mailing. The instruments can be made from these drawings by any laboratory possessing a mechanic. These drawings will not be supplied to apparatus-makers or other persons than psychologists except in special cases.

The following changes and corrections are to be made in the *Elementary course in psychological measurements* published in these Studies, 1897 IV.

Page 89, in line 9 from bottom read "for about two or three hours each."

Page 102, the formulas in the middle of page should read

$$A = \frac{A_r + A_l}{2}, B = \frac{B_r + B_l}{2}, \text{ etc.}$$

Page 122, in lines 3 and 7 from top, and in line marked *D*, read "Ex. IX." instead of Ex. VI.

Page 122, lines 8 and 9 from top should read: sent through the break contact of the key and the explained in Ex. IX; the condenser is connecte usual."

Page 122, in the line marked *G* insert 7^{σ} after th

Page 123, in the last line of the specimen reco has been omitted in two cases; they should read —4.(— 46 and — 87 respectively.

Page 129, the last lines of paragraph *p* should rea contact at 80°, then at 120° and then at 160°; draw

Page 138, in line 8 from the bottom change ½

Page 139, in line 21 change ½*m* and ½*n* to "1

Page 139, in line 16 change 2*m* and 2*n* to "1/2*n*